THE
PHILOSOPHICAL MAGAZINE
AND JOURNAL:

BY ALEXANDER TILLOCH,

M.R.I.A. F.S.A. Edin. and Perth, &c.

Ne, argentorum accipe pretium idem statim, quin et alienis libraria perquae.

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OF THE FORTY-EIGHTH VOLUME.

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I. *Some brief Particulars respecting the Strand or Waterloo Bridge, erected over the Thames, at the Savoy, London.*

With our present number we have given an engraving, by Porter, of this elegant and highly useful embellishment of the metropolis *. The foundation stone was laid on the 8th of October 1811, and the ninth and last arch was finished on the 28th of November 1815, having thus occupied very little more than four years, a space during which no work of such magnitude was ever known to have been executed in ancient or modern times. Indeed many smaller works of the kind have been several years more in building.

The superstructure only remains to be executed; and when the avenues on the north side from the Strand through the Savoy, and on the south or Surrey side to the Obelisk in St. George's fields, are made, the bridge will be opened for carriages and foot passengers promiscuously. This part of the work, however, will require great skill; and cannot be executed without a most serious expense: it will require much ingenuity, for instance, to make the road on the Surrey side of the river, as it is intended, for the sake of easy ascent, that the road shall pass over a series of arches almost as far as the Obelisk; while on both sides of the river the compensations demanded of the Strand Bridge Company for the buildings necessary to be pulled down are enormous beyond all precedent. Eight hundred thousand pounds have been already expended, and the least cost of the completion of the design will be two hundred thousand pounds more, which we trust there will be no difficulty in raising.

The whole structure is of beautiful Cornish granite, and the arch stones are mostly from 5 to 7 tons in weight. Blackfriars bridge, as our readers already know, is of Portland stone; and it

* To avoid too small a scale, only one half of the structure occupies the plate: the other half being precisely the same.

is highly to the credit of the Strand or Waterloo Bridge Company that they have adopted the former more durable material; for, had mere profit been their object, the Portland stone would certainly have been preferred for its cheapness.

We need scarcely add that this magnificent structure is a theme of admiration for beauty of form and exquisite masonry to all persons of taste or skill in architecture. The celebrated Canova in particular, when lately in London, pronounced it to be the grandest work of the kind in the world.

II. *The original Inhabitants of America shown to be of the same Family and Lineage with those of Asia; by a Process of reasoning not hitherto advanced. By* SAMUEL L. MITCHILL, *M.D., Professor of Natural History in the University of New-York; in a Communication to* DE WITT CLINTON, *Esq. President of the New-York Philosophical Society, dated New-York, March* 31, 1816*.

THE view which I took of the varieties of the human race, in my course of Natural History, delivered in the University of New-York, differs in so many particulars from that entertained by the great zoologist of the age, that I give you for information, and without delay, a summary of my yesterday's lecture to my class.

I denied, in the beginning, the assertion that the American aborigines were of a peculiar constitution, of a race *sui generis*, and of a copper colour. All these notions were treated as fanciful and visionary.

The indigenes of the two Americas appear to me to be of the same stock and genealogy with the inhabitants of northern and southern Asia. The northern tribes were probably more hardy, ferocious and warlike, than those of the south. The tribes of the lower latitudes seem to have been greater proficients in the arts, particularly of making clothes, clearing the ground, and erecting works of defence.

The parallel between the people of America and Asia affords this important conclusion, that on both continents the hordes dwelling in the higher latitudes have overpowered the more civilized though feebler inhabitants of the countries situated toward the south. As the Tartars have overrun China, so the .... As the Huns and Alans destroyed .... and Iroquois prostrated the populous .... of the Ohio.

The surviving race in these terrible conflicts between the different nations of the ancient native residents of North America,

is evidently that of the Tartars. This opinion is founded upon four considerations.

1. The similarity of physiognomy and features. His excellency M. Genet, late minister-plenipotentiary from France to the United States, is well acquainted with the faces, hues and figures of our Indians and of the Asiatic Tartars; and is perfectly satisfied of their mutual resemblance. Mons. Cazeaux, consul of France to New-York, has drawn the same conclusion from a careful examination of the native man of North America and Northern Asia.

Mr. Smibert, who had been employed, as Josiah Meigs, esq. now commissioner of the land office of the United States relates, in executing paintings of Tartar visages, for the grand duke of Tuscany, was so struck with the similarity of their features to those of the Naraganset Indians, that he pronounces them members of the same great family of mankind. The anecdote is preserved, with all its circumstances, in the fourteenth volume of the Medical Repository.

Within a few months I examined over and again seven or eight Chinese sailors, who had assisted in navigating a ship from Macao to New-York. The thinness of their beards, the bay complexion, the black lank hair, the aspect of the eyes, the contour of the face, and in short the general external character, induced every person who observed them, to remark how nearly they resembled the Mohegans and Oneidas of New-York.

Sidi Mellimelli, the Tunisian envoy to the United States in 1804, entertained the same opinion, on beholding the Cherokees, Osages, and Miamies, assembled at the city of Washington during his residence there. Their Tartar physiognomy struck him in a moment.

2. The affinity of their languages. The late learned and enterprising Professor Barton took the lead in this curious inquiry. He collected as many words as he could from the languages spoken in Asia and America; and he concluded, from the numerous coincidences of sound and signification, that there must have been a common origin.

3. The existence of corresponding customs. I mean at present to state that of shaving away the hair of the scalp, from the fore-part and sides of the head, so that nothing is left but a tuft or lock on the crown.

The custom of smoking the pipe, on solemn occasions, to the four cardinal points of the compass, to the heavens and to the earth, is reported, upon the most credible authority, to distinguish equally the hordes of the Asiatic Tartars and the bands of the American Siaux.

4. The kindred nature of the Indian dogs of America and the Siberian dogs of Asia.

The animal that lives with the natives of the two continents, as a dog, is very different from the tame and familiar creature of the same name in Europe. He is either a different species, or a wide variety of the same species. But the identity of the American and Asiatic curs is evinced by several considerations. Both are mostly white. They have shaggy coats, sharp noses and erect ears. They are voracious, thievish, and to a considerable degree indomitable. They steal whenever they can, and sometimes turn against their masters. They are prone to snarl and grin, and they have a howl instead of barking. They are employed in both hemispheres for labour: such as carrying burthens, drawing sleds over the snow, and the like: being yoked and harnessed for the purpose, like horses.

This coincidence of our Indian dog with the *Canis Sibericus* is a very important fact. The dog, the companion, the friend or the slave of man in all his fortunes and migrations, thus reflects great light upon the history of nations and of their genealogy.

II. The exterminated race in the savage encounters between the nations of North America in ancient days appears clearly to have been that of the Malays.

The bodies and shrouds and clothing of these individuals have within a few years been discovered in the caverns of saltpetre and copperas within the states of Kentucky and Tennesee; their entire and exsiccated condition has led intelligent gentlemen who have seen them to call them *mummies*. They are some of the most memorable of the antiquities that North America contains. The race or nation to which they belonged is extinct; but in preceding ages occupied the region situated between lakes Ontario and Erie on the north, the gulf of Mexico on the south, and bounded eastwardly by the Alleghany mountains, and westwardly by the Mississippi river.

That they were similar in their origin and character to the present inhabitants of the Pacific Islands and of Austral Asia, is argued from various circumstances.

The sameness of texture in the plain cloth or matting that . . . the mummies, and that which our navigators bring . . . islands, and the Fegees.

. . . there is between the feathery . . . from the islands of the South Sea, and . . . surround the mummies lately . . . The plumes of birds are twisted . . . peculiar skill, and turn water like the back

3. Meshes of nets regularly knotted and tied, and formed of a strong and even twine.

4. Mockasons or coverings for the feet, manufactured with remarkable ability, from the bark or rind of plants, worked into a sort of stout matting.

5. Pieces of antique sculpture, especially of human heads and of some other forms, found where the exterminated tribes had dwelt, resembling the carving at Otaheite, New Zealand, and other places.

6. Works of defence, or fortifications, overspreading the fertile tract of country formerly possessed by these people, who may be supposed capable of constructing works of much greater simplicity than the morais or burial-places, and the hippas or fighting-stages of the Society Islands.

7. As far as observations have gone, a belief that the shape of the skull and the angle of the face in the mummies correspond with those of the living Malays.

I reject therefore the doctrine taught by the European naturalists, that the man of Western America differs in any material point from the man of Eastern Asia. Had the Robertsons, the Buffons, the Raynals, the De Pauws, and the other speculators upon the American character and the vilifiers of the American name, procured the requisite information concerning the hemisphere situated to the west of us, they would have discovered that the inhabitants of vast regions of Asia, to the number of many millions, were of the same blood and lineage with the undervalued and despised population of America. The learned Dr. Williamson has discussed this point with great ability.

I forbore to go further than to ascertain by the correspondences already stated, the identity of origin and derivation to the American and Asiatic natives. I avoided the opportunity which this grand conclusion afforded me, of stating that America was the cradle of the human race; of tracing its colonies westward over the Pacific Ocean, and beyond the sea of Kamschatka, to new settlements; of following the emigrants by land and by water, until they reached Europe and Africa; and lastly, of following adventurers from the former of these sections of the globe to the plantations and abodes which they found and occupied in America. I had no inclination to oppose the current opinions relative to the place of man's creation and dispersion. I thought it was scarcely worth the while to inform an European, that on coming to America, he had left the *new* world behind him for the purpose of visiting the OLD. It ought, nevertheless, to be remarked, that there are many important advantages derived to our reasoning from the present manner of considering the subject. The principles being now established, they will be supported

ported by a further induction of facts and occurrences, to an extent and an amount that it is impossible, at this moment, fairly to estimate. And the conclusions of Jefferson, Lafon, and others favourable to the greater antiquity of American population, will be daily reinforced and confirmed.

Having thus given the history of these races of man, spreading so extensively over the globe, I considered the human family under three divisions:

First, the TAWNY man, comprehending the Tartars, Malays, Chinese, the American Indians of every tribe, Lascars, and other people of the same cast and breed. From these seemed to have proceeded two remarkable varieties ; to wit,

Secondly, the *White* man, inhabiting naturally the countries in Asia and Europe situated north of the Mediterranean Sea ; and, in the course of his adventures, settling all over the world. Among these I reckon the Greenlanders and Esquimaux.

Thirdly, the *Black* man, whose proper residence is in the regions south of the Mediterranean, particularly toward the interior of Africa. The people of Papua and Van Diemen's Land seem to be of this class.

It is generally supposed, and by many able and ingenious men too, that external physical causes, and the combination of circumstances which they call climate, have wrought all these changes in the human form. I do not, however, think them capable of explaining the differences which exist among the nations. There is an internal physical cause of the greatest moment, which has scarcely been mentioned. This is the generative influence. If by the act of modelling the constitution in the embryo and foetus, a predisposition to gout, madness, scrofula, and consumption, may be engendered, we may rationally conclude, with the sagacious D'Azara, that the procreative power, may also shape the features, tinge the skin, and give other peculiarities to man.

Yours truly,

SAMUEL L. MITCHILL.

III. *Notes of a meteorological Tour ; by* THOMAS FORSTER,
F. S. in company with J. J. PEARSON,

April ... Lowering morning, but no rain ... Nimbus cloud and stackencloud. We left ... in the evening and proceeded to.

... day. Fine still evening. The jackdaws flying round ... flocks, and very clamorous about the ruins of the old
abbey,

abbey. The lower atmosphere was damp and hazy. Swallow (*Hirundo rustica*) first seen, and the bat seen.

April 27.—We left Linlithgow at 5 A.M. Fair morning and warm, with curlcloud, and some stackenclouds, &c. I had occasion to notice a large number of swallows from the tower of Doon Castle. We arrived to dinner at Callender in the Highlands. I noticed in the evening, after a fair day, that the *cumuli* which had previously only lodged on the mountain tops, descended lower and lower, till I saw them dissolve into fallcloud, or fog, and fill the valleys. I never had an opportunity of seeing this process so perfect before. The squalling of the peacocks in the evening, and the increase of clouds aloft, indicate rain. Ben Leddi is still covered at the top with snow, and few trees have any appearance of verdure yet. There is a striking similarity between the general face of the country here and that of part of Merionethshire and other parts of North Wales. There is said to be a great resemblance between the inhabitants of mountainous countries, but I observe a striking difference between the Highland Scotch and the North Welch. The former have a greater disposition to acquire knowledge, but are more circumspect, and not of so warm a disposition in general.

April 28.—*Cumuli* creeping up the hills. They were capped with clouds all the way we travelled by Loch Ern to Crieff. Some of these clouds however, were higher, and there were features of sondercloud and curlcloud higher up. The manners of the people change very much as one advances from the capital towards the Highlands. The dress too of the Highlanders, with their plaids and kilts. The style of building of the old towns resembles somewhat that of the old towns on the French coast. We saw water-birkers about the lakes, also the water ouzel.

April 29.—Clouds on the hills, which then continued showers of gentle rain. We proceeded to Perth—Here again the manners of the people are more anglicised. The tramclouds and rainclouds rested on hills not more than 1000 feet high, which is very low for these clouds.

April 30.—Leaving Perth early, I noticed the lowness of the clouds. Their height cannot be so well found in a flat country, as here where they are seen interposed between hills of known height. The rainclouds or *nimbi* skirting the sides of the hills through the valleys often did not condense into drops of water, till within 2 or 300 feet of the ground. We passed through Dunkeld and Cupar of Angus, and slept at Forfar. The afternoon was very rainy. We observed abundance of gulls, particularly the black-headed gull on the ploughed grounds near Cupar. I saw the martin, for the first time this year, flying over the Tay by Dunkeld's bridge.

May 1.—Clouded morning with a mist. I noticed to-day
between Laurencekirk and Aberdeen a curious phænomenon, not
very common in England. The *cumuli* in many places rolled
along with their bases on the flat ground, involving us occasion-
ally in obscurity, and there being to all appearance a fog, but in
the profile at a distance I saw their cumulated forms. I noticed
also the vapour condensed in many places about a foot from
ploughed ground, so as to appear like little *strati* or fallclouds,
forming here and there close to the earth. I should observe
that the *cumuli* touching the ground were going into the nimbi-
form state, and that others at some distance were actually con-
densing into small rain a few feet from the ground. When we
got to Aberdeen a white and wet mist was involving the whole
town and its neighbourhood. In the fine intervals by day, the
different modifications were seen in very indistinct features.

May 2.—Fine spring morning, but no leaves on the trees yet.
Light gales. *Cumuli* and the lighter modifications higher up.
We left Aberdeen, and passing through Ellon came into Peter-
head at night. Fleecy *cumuli* evaporating, and the new moon
seen at intervals. In passing along we saw various kinds of gulls
and the heron flying after sunset. Among other birds I saw the
curlew (*Scolopax arcuata*) flying over the town of Ellon.

May 3.—Left Peterhead again almost immediately after taking
refreshment, about one in the morning, and passing through Cul-
len, Fochabers, Elgin, Ferres, and Nairne, arrived at Inverness
late at night. The country through which we passed was bar-
ren, except a few large plantations of pines and firs. The
weather was showery with wind, and the air cool, with not the
least appearance of spring. The inhabitants of the towns through
which we passed were very different in appearance from the High-
landers. On inquiring at the different booksellers in our route,
what works on the subject of meteorology were known here, I
found that the works of Professors Leslie[*] and Jamieson[†] were
the only scientific works, relating to this science, known in this
part of the country.

May 4.—Fine day, with various clouds; much *cirrostratus* or
wane cloud forming towards night.

May 5. (*Inverness*.)—I had an opportunity this morning
of observing the formation of matutinal *cumuli* or stackenclouds
on the tops of the mountains, at a time when others in a
lower region were producing rainclouds by inosculation. These

little

little *cumuli* formed in the valleys had the hills for a background, and were very distinctly seen. They manifested the same process as *cumuli* which form high up over flat countries; many small ones being lost as if by evaporation, while the larger were increased and crept up the hills and got aloft. Gentle rain came on in light showers, with warm air. The spring begins to appear. The distances were clear, and I could distinctly ken Fort George as I crossed the water into Ross-shire. By comparing the account of persons with whom I have conversed, I notice the average temperature is greater here than at Aberdeen.

May 6.—I noticed some nascent *cumuli* on the hills, the upper atmosphere being also thickly clouded. In general, to-day, the clouds were higher.

We travelled along the western margin of Loch Ness to

FORT AUGUSTUS.—Showers came on in the evening, but I observed that the streams of rain were formed higher up in the air than was the case yesterday morning. Walking out among the mountains at night after the rain, I heard the shrill cry of some waterfowl near the loch. The heron was sailing leisurely over the loch as we passed along this evening, not far from the Castle of Urquhart, whose ruined towers are mouldering away by degrees, and their stones rolling down the precipices into the loch below.

May 7.—Proceeded to FORT WILLIAM. I had occasion to observe a range of *cumuli* (approaching in nature to *cumulo-stratus*) in the valleys below Ben Nevis, and the contiguous mountains extending irregularly upwards. Some portions were detached, and went up the sides of the mountain. Higher up were *cumuli* of more scudlike and nimbiform texture; during this time it was raining, a circumstance which enabled me to see the formation of those nascent clouds which feed showers from below, during the actual falling rain. Viewed with the high hills of Ben Nevis and its range of mountains behind them as a background, I could distinctly see and compare the difference in colour and density of these clouds and nimbiform clouds higher up. I noticed too that certain flocculous masses of thin clouds apparently evaporated, and the greater masses increased, while some portions seemed attached to the tops of the hills, which were comparatively clear. In the evening while rowing on the water, I saw the clouds rolling along much below very low hills, while others which sat upon higher ones had fine cumulous tops, and a nimbiform or dense wanecloud much higher up moved off with the direction of the wind and left a dingy horizon at sunset. I found ascending to the top of Ben Nevis quite impossible from the depth of snow. Herring gulls flying in flocks about the water. Air mild. Spring advancing. By night

night the sky was clouded with large clouds in different altitudes. I had to day a fine opportunity of confirming, by processes I saw going on about the mountains, the opinion I have long had of the electrical agency concerned in producing the forms of clouds, of which I shall treat more largely elsewhere.

May 8.—Left Fort William early, and crossing the ferry, proceeded through Glenco to Tyndarum. The clouds still on the hills, and gentle showers in the morning. I observed to-day one of the peaked mountains of Argyleshire involved about its base in small rain, while its pointed summit was capped with the raincloud in a less watery state. What struck me as remarkable was this, that the cloud did not terminate in rain by degrees, but there seemed a distinct boundary between the cloud at the top, and the rain surrounding its lower part and descending to the ground. In the evening most of the clouds were much higher, and the air got sharper. I met Hogg the Etick shepherd and poet at a small public-house near the glen.

May 9.—We proceeded to Stirling by way of Callender; the weather cold, cloudy, and windy.

May 10.—Returned to Edinburgh. Cold, windy, and cloudy day.

May 11.—(At Edinburgh.) The weather still bearing the character of winter. Hard showers of snow, hail and sleet, with fair intervals. The frondescence is very little advanced yet; and an Englishman would suppose it March instead of May.

May 12.—Fair day, with common dayclouds, and some features of the light modification, particularly the *cirrus* in the morning. The air warmer than yesterday. The swallows are by no means common yet, only a few stragglers here and there.

May 13.—Left Edinburgh, and came at night to Jedburgh, by way of Melrose. Fair morning, and clouds high; cold night.

May 14.—I proceeded to Carlisle; the weather cold in the shade with the diurnal stackenclouds. I saw the *Hirundo urlica* at the abbey, and first saw and heard *Hirundines* upon this place.

May proceeded by Keswick to Ambleside *. Fair morning, watterclouds; in the evening rain among the mountains, way along Ulswater to Ambleside. I noticed at passing, that being high on the hill there was no rain, but was in mist, or cloud scarcely wetting me. As I got lower it became small drops of rain, producing however a kind of fog; and when I got to the valleys below it was fair rain in streams. The watery particles seem to get larger and larger rainstreams in descending.

* The best drawn clouds of any I have seen, were those shown me by ——, an artist of Ambleside.

May 16.—Dr. Spurzheim joined me at Ambleside. The morning was warm, and the spring advancing rapidly. Towards three in the afternoon rain came on in continued showers, and lasted all the evening. Gusts of wind at night.

May 17.—Walked from Ambleside in Westmoreland to Keswick in Cumberland. Chiefly cloudy. Helvellyn was cloud-capped all the morning; and at length the cloud hanging over its sides seemed to descend in rain, whose streams acquired progressive magnitude in descending. In the evening I noticed the clouds getting higher. There arose also clouds in the valleys and from the sides of the hills which crept up them and passed off in the form of *cumuli* of the floccose kind. In the same manner the nourishing *cumuli* of showers form and feed them from below. I had thus an opportunity of observing distinctly the formation of *cumuli* in the eventide, and of their rising upwards, as an indication of the fine weather of the following day.

May 18. (At Keswick.)—Early in the morning high masses of cloud showed a sondercloudiness of form; a fine day with light *cumuli* followed and some small evaporating *cirri*. Towards evening the *cumuli* enlarged, with continuous common bases, and obscured the top of Skiddaw. I noticed several of the green cormorants and wild ducks while I rowed on the lake called Derwentwater.

May 19. Sunday. (At Keswick.)—A fine day, we ascended the mountain Skiddaw; on the summit whereof the *cumuli* lodged. In the evening the clouds were high, but in greater quantity. When on Skiddaw in the morning, I noticed *cirro-stratus* in the distance at nearly the same altitude as we were: but it was hanging over level grounds. I also observed that when the *cumuli* got a continuity of base with greater density, becoming *cumulostratus* they lodged on the mountains; but afterwards simple *cumuli* became ragged and descended down by the sides of the mountain in particular places.

May 20.—Proceeded to Penrith in the afternoon, after sailing on the lake. Clear day, cold in the wind, and hot in the sun, with *cumuli* and light clouds in general.

May 21.—I came to Manchester by Kendal. Fine morning and cloudy night, with some rain.

May 22. (At Manchester.)—Dry dusty clear day, *cirro-stratus*, &c.

May 23.—Went by way of Chester to St. Asaph in North Wales. Fine clear day and night.

May 24.— Travelled through Denbeigh and Llangollen to Oswestry. Fine still spring morning, and very warm. Clouded over with a breeze at night.

May 25.—Proceeded to Bath. Weather showery and cooler again.

May 26.—(At Bath.)—Fine day, with *cumuli*, &c.

May 27.—Fine day, with *cumulus* and other clouds. Clouded over in the afternoon.

May 28 —Fair, with some showers ; fine spring weather.

May 29.—Fair spring weather, with nimbification, &c.

May 30.—Fine warm weather.

May 31.—Fair day, with various clouds.

June 1.—Left Bath and returned to Clapton. Fine warm spring day, the morning was particularly clear.

IV. *Some Observations and Experiments made on the Torpedo of the Cape of Good Hope in the Year* 1812. *By* JOHN T. TODD, *late Surgeon of His Majesty's Ship Lion. Communicated by Sir* EVERARD HOME, *Bart. V.P.R.S.**

Whilst the Lion was stationed at the Cape of Good Hope, the seine, as is the custom throughout the navy, was frequently employed in procuring fish for the use of the ship's company, and besides the more edible kinds, many of the torpedo were caught. In this manner the opportunity was afforded me of making the following observations, some of the imperfections of which I must be allowed to attribute to the *"manus nuda"* of my situation. The fish were generally caught early in the morning, and examined as soon after as possible. When this could not be done, they were placed in buckets of sea-water, where they sometimes remained alive for three, and in one instance for five days.

The torpedo is seldom met with to the eastward of the Cape of Good Hope. Hence, whilst I rarely failed in procuring them in Table Bay, I never but once succeeded in doing so in Simon's Bay, although the opportunities were the same in both places. It was never caught but by the seine, although the hook and line, with bait of every variety, were as often made use of exactly in the same situations. It differs in no respect, as far as I have been able to observe, from the same fish of the northern hemisphere, except that it was never found so large ; being never more than eight, nor less than five inches in length, and never more than five, nor less than three inches and a half in breadth. The colour of the animal is various ; the upper surface being generally hazel grey, reddish brown, or purple; the under surface greyish white, yellowish white, or white with black patches.

* From the Philosophical Transactions for 1816, part i.

The

The columns of the electrical organs were larger, and less numerous in proportion, than those described by Mr. Hunter, in the torpedo caught at La Rochelle. When separate and uninfluenced by external pressure, they appear to be of the form of cylinders, as is shown as nearly as possible by suspending them by one of their extremities. The different forms which they exhibit in a horizontal section of the whole organ, are produced by their unequal attachment to one another by the intermediate reticular substance

The electrical organs are so placed within the curvature of the semilunar cartilages of the large lateral fins, as to be entirely under the influence of the muscles, which are inserted into these cartilages. So that in any lateral motions of these cartilages towards the trunk, or in any increase of curvature of these cartilages, the electrical organs must be compressed. There appears also to be a muscular structure, which connects the anterior part of these cartilages to a process projecting from the anterior part of the cranium, the action of which must tend to increase this effect.

The inferior and posterior terminations of the small lateral fins are covered with laminæ of osseous matter, which are enveloped in the epidermis.

A much larger proportion of nerves is supplied to the electrical than to any other organs. This has appeared to others so important an observation, that it may be repeated with propriety.

The shocks received from the torpedos which I examined, were never sensible above the shoulder, and seldom above the elbow-joint. The intensity of the shock bore no relation to the size of the animal (sensation being the only measure of intensity), but an evident relation to the liveliness of the animal, and *vice versâ.* The shocks generally followed simple contact, or such irritation as pressing, pricking or squeezing, sometimes immediately, and sometimes not until after frequent repetition. Not unfrequently, however, animals apparently perfectly vigorous suffered this irritation without discharging any shock. There appeared no regularity of interval between the shocks. Sometimes they were so frequent as not to be counted; at other times, not more than one or two have been received from one animal; and, in a few instances, it has been impossible by any irritation to elicit shocks from some of them. When caught by the hand, they sometimes writhed and twisted about, endeavouring to extricate themselves by muscular exertion, and did not, until they found these means unavailing, discharge the shock. In many instances, however, they had recourse to their electrical power immediately.

The electrical discharge was, in general, accompanied by an
evident

evident muscular action. This was marked by an apparent swelling of the superior surface of the electrical organs, particularly towards the anterior part, opposite to the cranium, and by a retraction of the eyes. It was so evident, that when the animal was held in the hand of another person, I was often able to point out when he received the shock. In this, however, I was also sometimes deceived; and I think I have received shocks (particularly when the animal has been debilitated, and the shocks weak), without having been able to observe this muscular action.

Two of these animals, as nearly alike in every circumstance as possible, being each placed in a separate bucket of sea-water, from one of them frequent shocks were elicited by irritation, viz. simple contact, or pricking, &c; the other was allowed to remain undisturbed. The former became languid, the intensity of its shocks diminished, and it soon died; the last shocks being received in a continued succession, producing pricking sensations never extending above the hand. The latter continued vivacious, and lived until the third day. This experiment was frequently repeated with the same results; and it might be observed, in general, where there was no direct comparison made, that those which parted with the shocks most freely soonest became languid, and died; and those which parted with them most reluctantly, lived the longest.

Two torpedos being placed exactly in the same circumstances as the last mentioned, from one shocks were elicited until it became debilitated. It was then allowed to remain until the following day. When they were both examined, it was found that the animal from which no shocks had been previously received, discharged them very freely; but it was with the greatest difficulty that they could be procured from the other.

Having made an incision on each side of the cranium and gills of a lively torpedo, I pushed aside the electrical organs, so as to expose and divide their nerves. The animal was then placed in a bucket of sea-water. On examining it in about two hours afterwards, I found it impossible to elicit shocks from it by any irritation, but it seemed to possess as much activity and liveliness, and lived as long as those animals from which shocks had not been received, and which had not undergone this change.

Two of these animals being procured, the nerves of the electrical organs of one of them were divided after the manner above described. They were placed each in separate buckets of sea-water, and allowed to remain undisturbed. This was performed in the morning, and when examined in the evening, it was impossible to distinguish between the liveliness or activity of either.

Of

Of two of these animals, the nerves of the electrical organs of one of them were divided. Being placed each in separate buckets of sea-water, they were both irritated as nearly alike as possible. From the perfect animal, shocks were received; after frequent repetition it became weak, and incapable of discharging the shock, and soon died. The last shocks were not perceptible above the second joint of the thumb, and so weak as to require much attention to observe them. From the other no shocks could be received; it appeared as vivacious as before, and lived until the second day. This experiment was frequently repeated with nearly the same results.

The nerves of one electrical organ only being divided in a lively torpedo, from which shocks had been previously received, on irritating the animal it was still found capable of communicating the shock. Whether there was any difference in the degree of intensity could not be distinctly observed. One electrical organ being altogether removed, the animal still continued capable of discharging the electrical shock.

Having divided one of the nerves of each electrical organ in a torpedo, from which shocks had been previously received, I still found the animal capable, after this change, of communicating the shock.

Having introduced a wire through the cranium of a torpedo, which had been communicating shocks very freely, all motion immediately ceased, and no irritation could excite the electrical shock.

I never received a shock from a torpedo, when held by the extremities of the lateral fins or tail.

The preceding account appears to me to afford grounds for the following conclusions.

1. That the electrical discharge of this animal is in every respect a vital action, being dependent on the life of the animal, and having a relation to the degree of life and to the degree of perfection of structure of the electrical organs.

2. That the action of the electrical organs is perfectly voluntary.

3. That frequent action of the electrical organs is injurious to the life of the animal; and, if continued, deprives it wholly of it. Is this only an instance of a law common to all animals, that by long continued voluntary action they are deprived? Whence is the cause of the rapidity with which it takes place in this instance? Or is it owing to the reaction of the mind of the animal?

4. That those animals, in which the nerves of the electrical organs are intersected, lose the power of communicating the shock, but appear more vivacious, and live longer than those in which

which this change has not been produced, and in which this
power is exerted. Is the loss of the power of communicating
the shock to be attributed to the loss of voluntary power over
the organ? Does this fact bear any analogy to the effects pro-
duced by castration in animals?

5. That the possession of one organ only is sufficient to pro-
duce the shock.

6. That the perfect state of all the nerves of the electrical
organs is not necessary to produce the shock.

And, 7. From the whole it may be concluded, that a more
intimate relation exists between the nervous system and electri-
cal organs of the torpedo, both as to structure and functions,
than between the same and any organs of any animal with which
we are acquainted. And this is particularly shown, 1st, By the
large proportion of nerves supplied to the electrical organs:
and, 2d, By the relation of the action of the electrical organs
to the life of the animal, and *vice versá.*

V. *Reply to Dr.* Prichard *on the Cosmogony of Moses.* By F. E——s.

To Mr. Tilloch.

Sir, — With commendable prudence, Dr. Prichard passes
lightly over the tender ground of his inconsistent assertions ;
affirming, in allusion to the direct proof I had given of them, that
I am " still determined to find contradictions between proposi-
tions which have no relation to each other," and that I *quarrel*
" even with the words in which they are expressed." If these
two affirmations be not absolutely correct, they are, at least, in
correctness absolutely equal. He cannot " stay to notice *mere
cavils*," but hastens to exhibit " a *specimen* of the *mode* of rea-
soning adopted by" his " *pertinacious critic.*" I shall now en-
deavour to show whether what he has produced for that pur-
pose be a specimen of my logic or of his own fairness ; and in
this attempt as well as on similar occasions he will, I trust, have
the goodness to endure repetitions which he has rendered un-
avoidable.—" In my last paper," says he, " I hinted at the in-
stance of St. Matthew and St. Luke, in order to prove that *in-
spired writers have chosen to avail themselves* of historical do-
cuments, when such sources of information *were to be found.*
Mr. F. E. seems to allow the force of this example, but denies
that it leads to any inference with respect to Moses ; and the
exception he takes against it is to the following purport. St.
Matthew and St. Luke found pre-existing documents, which it
only

only required in them human sagacity to adopt; but Moses, it seems, had nothing but *the light of revelation to guide him;* consequently he made no use of records. Now there is," continues he, " *one grand objection* to this conclusion ; viz. that it takes for granted the chief thing intended to be proved." There is also, I would remind Dr. Prichard, *one grand objection* to the whole argument ; viz. that it is not exactly mine. My exception rests on the want of analogy in the cases : in the one, inspiration appears to be superfluous, in the other indispensable. The compilation of a genealogy, of which the materials were *we know* recorded in Scripture, needed no inspiration. The *manner* of the *creation* could be known *with certainty* to no human being, not even to Moses, except by *a revelation.* Besides, there is in Scripture neither hint nor trace of *any revelation concerning the creation, anterior to Moses;* nor is there any other ground for the belief of *inspired* documents or traditions of which he might have availed himself, than that of vague and visionary conjecture*. But waiving this consideration, and supposing that there may have existed in the time of Moses documents or traditions of such a primitive revelation ; there could, if he was inspired with regard to the creation, be no possible motive for his having recourse to them. In such circumstances it is quite idle to talk of an *inspired writer choosing to avail himself* of other authorities in preference to the immediate authority of Heaven. The version of my argument therefore, should not have been generally, that " Moses had nothing but the light of revelation to guide him; consequently he made no use of records:" but that, with regard to the creation, Moses having the light of revelation to guide him, there is no assignable reason why he should be made to have recourse to *assumed records* or *traditions.* I do not perceive that the *one grand objection* applies to this conclusion.

Dr. Prichard disclaims the merit which he erroneously conceives I intended to ascribe to him, *of having formed* the " *ingenious imagination*" of circuitous inspiration. I am, indeed, ignorant to whom this not recent invention is due; but as an *auxiliary* whom he esteems " perfectly well informed respecting the points in controversy†" had employed it in his favour, I confess that by showing its insufficiency in the case to which it was applied, I intended to prevent its adoption by the principal. Of his *protest* against the *pretensions* of those who, like me, " talk largely about circuitous inspirations, and immediate inspirations," I can merely express an unconsciousness of enter-

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* Dr. Prichard, however, esteems it most *unrelentingly inquisitorial* of me to require any better proof of this *assumed primitive revelation.*

† See Phil. Mag. No. 216, pp. 341 and 342.

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taining any *pretensions* to which it can allude. I acquiesce in
his estimate of the superior weight of " fair inference from his-
torical facts," to that of " hypotheses concerning *sorts* of in-
spiration." The points in discussion do not, however, require
that I should offer any opinion on the historical facts which de-
termine him to believe *that Moses was not the original author
of the Cosmogony;*" it is sufficient that I have shown some of
the consequences which result from such a supposition. I shall
only add, that his hypothesis, which gives the Hebrew, Hindoo,
and Etruscan Cosmogonies a common but unascertained origin
of higher antiquity than Moses, will scarcely be thought calcu-
lated to induce any very strong belief that the hidden source of
this common origin was Heaven.

I am charged with having unfoundedly *insinuated* that Dr.
Prichard rested his interpretation of the word *day*, solely on the
authority of Josephus and Philo. If what I have written admit
such a construction, I most explicitly disavow it; but if it be
found that what I said, so far from containing such an absurd
insinuation, contains no insinuation whatever, Dr. Prichard may
possibly retract his imputation. I had replied to *some* of his ar-
guments adduced in support of the figurative sense of the word
day, when he reminded me that I had overlooked the authority
of Josephus and Philo. My answer to this contained nothing
that it did not directly express : " If," said I, " he rest the me-
taphorical sense of the word *day* on *their* authority, he must
also on the same authority admit a figurative sense of the
whole first chapter of Genesis." Why this answer should have
the misfortune to displease him I know not, since he avows that
it has been his " endeavour to show that *every part* of the first
chapter of Genesis is *more or less* metaphorical." I really have
no distinct conception of the modification here intended by the
words " *more or less;*" but if " *every part*" be metaphorical,
the *whole* it may be presumed can be neither *more nor less* than
allegorical.

In his zeal to establish his *coincidences*, and to apply to some
useful purpose his discovery of the true sense of the 20th verse of
the first chapter of Genesis, Dr. Prichard with the stroke of a
pen deprived myriads of animals of *locomotion*. When it was
afterwards objected to him, that even his own version of this
verse " does not exclude testacea from the *fifth day's creation*,"
a considerable *portion* of the *order* being *indisputably* endued
with *locomotion;*" instead either of acknowledging or defending
his error, he is pleased to say, that he " shall not enter *further
into* the inquiry what place CORALS and bivalves hold in the scale
of creation, whether THEY ARE, AS F. E. DECLARES, LOCOMOTIVE
ANIMALS, or approach to the character of vegetables:" gravely
                                                        adding

adding that " the *question* has been *decided in his favour* by a third person," competent and impartial, to whose satisfactory remarks he refers. It is indeed true, that in the remarks referred to, there are unsupported assertions that the order of testacea is destitute of *locomotion:* if this, therefore, be sufficient, the question is certainly decided against me. Having, however, a presentiment, that, notwithstanding the " *crambe repetita,*" Dr. Prichard will condescend to honour this letter with some notice, I am desirous of appealing from this decision, *even to himself,* and would venture directly to ask him, *whether the whole order of testacea be destitute of locomotion* * *?* If on recollection, after consulting naturalists, it be found that an affirmative answer cannot be given, he will, it may be supposed, be constrained, at the expense of his *coincidences,* to restore at least a part of the *order* to the *fifth day's creation.* I have yet one other question, to which it more nearly concerns him to return an explicit answer. He peremptorily and tauntingly affirms, that I declare CORALS and bivalves are LOCOMOTIVE ANIMALS. Of the part of this assertion which relates to bivalves I ask for no explanation, though it may easily be perceived that some of them were meant to be included in the portion of the order of testacea excepted from being " *indisputably* endued with *locomotion* † ; but I am entitled to ask where Dr. Prichard found, either directly or even *by inference,* the declaration he attributes to me, that CORALS are LOCOMOTIVE ANIMALS? My assertion respecting locomotive animals (as has just been seen) instead of extending beyond, did not even include the *whole order of testacea;* and with regard to *corals,* as comprehended in zoöphytes, it will be seen that the *motion* which it cannot be

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* The genus *Janthina* (for example) " formed by Lamarck on a single shell described by Lister, Brown, Forskal, and other naturalists; which derives its claims of distinction from Helix, not so much from the character of the shell as from that of the *animal,* which differs in its *structure* materially from the animal of the Helix, since it is *furnished* with *a curious apparatus* (being an inhabitant of the sea) *for swimming,* instead of that for *crawling,* with which the *Helices are provided.*"—See Nicholson's Encyclopædia, article *Shell.*

† Some bivalves, *even* of the *genus Ostrea,* give indisputable proofs of locomotive powers. " *Scallops leap out of the water to the distance of half a yard.*" (Nicholson's Encyclopædia.) Speaking of the same bivalve, Burrow in his Conchology, page 80, says, " Strong locomotive powers have been attributed to the Pecten, which are, it is said, exerted in a most singular manner. *A very rapid progress is effected* by the sudden opening and closing of the shell. This is done with so much muscular force as to throw it four or five inches at a time. In the water an equal dexterity is evinced by the animal, in raising himself to the surface, directing his course *ad libitum,* and suddenly, by the shutting of his valves, dropping to the bottom."

denied

denied they possess, was admitted to be different from *locomotion*.
As Dr. Prichard had not displaced *forest trees, shrubs* and *li-
chens* from the *third day's creation*, although in *strictness none
of them* come under the description of "*grass, seed-bearing
herbs*, or *fruit-bearing trees:*"—"on the *same principle*," I
observed, " it may be thought that zoöphytes might be permitted
to remain in the fifth day's creation, being *moving creatures that
have life*, although *their motion* does not *precisely accord* with
the *idea* which, in opposition to the received translation, *Dr.
Prichard thinks the Septuagint and the original convey.*"   So
far is this passage from containing any assertion of the *locomo-
tion of* zoöphytes, that it contains two admissions of the con-
trary. First, it is admitted by inference, that they do not *in
strictness* come under the description of *locomotive animals;*
and afterwards it is acknowledged that their *motion* does not
precisely accord with the *idea* (*of locomotion*) which Dr. Pri-
chard thinks the original text of Genesis conveys."

A subtle or complex argument may, in consequence of mis-
conception, be misrepresented; but of the process by which a
simple assertion is converted into a declaration totally different,
I do not pretend to anticipate the explanation.

I am, sir,

Your very obedient servant,

Bath, July 10, 1816.   F. E——s.

VI. *On the Diving-Bell, as a Means for curing Deafness.   By
Dr.* HAMEL, *of St. Petersburgh.*

To Mr. Tilloch.

SIR, — WHEN visiting the harbour which is now building at
Howth, near Dublin, I wished to make myself acquainted with
the manner in which the diving-bell for constructing the mason-
work under water is used, and obtained permission to descend.

When the mouth of the bell was about two feet under wa-
ter, I began to feel pain in both ears, occasioned by the pres-
sure of the condensed air against the membrana tympani; the
air in the inner cavity of the ear being of less density. Fear-
ing that this pain might become troublesome at a considerable
depth, I made exertions to admit air through the Eustachian
tube into the ear. I happened to accomplish this at first only
on one side, the air rushed into the cavity of the right ear, and
the pain ceased instantly. As the bell continued going down,
the pain returned; but as I repeated my exertions to open the
Eustachian tube, the air at intervals found its way through it,
and

and re-established the equilibrium. Through the left Eustachian tube no air had yet passed, and the pain in the left ear was gradually increasing. When about fourteen feet under water, the sensation was as if a stick was forced into that ear from without. At last, during one of the exertions to open the mouth of the Eustachian tube on that side, the air forced its way with considerable violence through it, and I was relieved of the pain also on that side.

After examining the mason-work of the pier at the bottom of the sea, I began to ascend. Here I soon felt pain again, resulting from the air in the inner cavity of the ear expanding, as the external pressure diminished: but this pain was more easily relieved, the air gushing at very short intervals without any voluntary exertions in small portions from the ear through the Eustachian tube into the mouth.

Noticing this, it occurred to me, that the diving-bell might be used for curing deafness in those cases where it depends on an obstruction of the Eustachian tube. The patient would have to go down in a diving-bell and make those exertions which open the mouth of the Eustachian tube, and then the pressure of the condensed air would force its way through the extent of the tube, and by that means clear the passage. It is well known, that slight obstructions have been frequently removed, by forcing air or tobacco-smoke from the cavity of the mouth into the ear. When I was in the diving-bell, and made my exertions to admit air into the Eustachian tube, I was not aware of the simple way in which it is effected. Dr. Wollaston informed me, that nothing is wanted but to swallow the saliva, as may be seen from the following simple and easy experiment. Close your nostrils with your fingers and suck with your mouth shut; air will come through the Eustachian tube from the ear, and you feel pressure on the membrana tympani, which prevents you from hearing distinctly. As the end of the Eustachian tube nearest to the mouth acts like a valve, this sensation will often remain even after you have ceased sucking. To remove it, nothing is wanted but to swallow saliva, whereby the action of the muscles seems to open the end of the Eustachian tube, and then the air rushes in to re-establish the equilibrium. This experiment shows on a small scale part of what I experienced in the diving-bell; where, to admit the air into the ear, one must merely swallow saliva, and at one of these exertions the air will rush in, if the obstruction be not very considerable.

I was anxious to know, whether men that had been in diving-bells before, had noticed the same sensations as myself, and I made them describe to me what they had felt. Among other things they stated, that when at a great depth it had been to them as

if a shot was fired through their brain." This was evidently the rushing in of the air through the Eustachian tube.

There are now several diving-bells in use. Besides the one at Howth, there is one at Holyhead, one at Ramsgate, and one at Plymouth. They are constructed under the superintendance of .Mr. Rennie, on Mr. Smeaton's plan, entirely of cast iron, in the form of an oblong chest open at bottom.—(See Dr. Brewster's Encyclopedia, article *Diving Bell.*)

The one in which I descended is six feet long by four feet wide, and six feet high, with twelve patent glass-lights, as used in ships' decks, on the top. A descent in a diving-bell of this construction may be undertaken without any inconvenience, except the above described sensation in the ears. I was for half an hour under water more than twenty feet deep, and had light more than enough to write and read. A constant supply of fresh air is given by means of a forcing pump, and the respiration is not in the least affected. The signals to the men, who manage the bell above the water, are given by means of striking with a hammer once, twice or more times against the inside of the bell. The number of strokes tells them in what direction you wish to be moved. A diving-bell of the above dimensions may hold four men.

I wish much that some deaf person or persons, whose deafness is owing to the cause above stated, might try the diving-bell; and should they be benefited by it, hydraulic or other pressure engines might be constructed to obtain the same end in houses or hospitals.

VII. *Further Experiments on the Combustion of explosive Mixtures confined by Wire-gauze; with some Observations on Flame. By Sir* H. Davy, *LL.D. F.R.S. V.P.R.I.*[*]

I have pursued my inquiries respecting the limits of the size of the apertures and of the wire in the metallic gauze, which I have applied to secure the coal miners from the explosions of fire-damp. Gauze made of brass wire $\frac{1}{70}$ of an inch in thickness, and containing only ten apertures to the inch, or 100 apertures in the square inch, employed in the usual way as a guard of flame, did not communicate explosion in a mixture of one part of coal gas and 12 parts of air, as long as it was cool; but as soon as the top became hot, an explosion took place.

A quick lateral motion likewise enabled it to communicate explosion.

Gauze made of the same wire, containing 14 apertures to the

inch,

inch, or 196 to the square inch, did not communicate explosion till it became strongly red hot, when it was no longer safe in explosive mixtures of coal gas; but no motion that could be given to it, by shaking it in a close jar, produced explosion.

Iron wire-gauze of $\frac{1}{10}$, and containing 240 apertures in the square inch, was safe in explosive mixtures of coal gas, till it became strongly red hot at the top.

Iron wire-gauze of $\frac{1}{60}$, and of 24 apertures to the inch, or of 576 to the square inch, appeared safe under all circumstances in explosive mixtures of coal gas. I kept up a continual flame in a cylinder of this kind, eight inches high and two inches in diameter, for a quarter of an hour, varying the proportions of coal gas and air as far as was compatible with their inflammation; the top of the cylinder, for some minutes, was strongly red hot; but though the mixed gas was passed rapidly through it by pressure from a gasometer and a pair of double bellows, so as to make it a species of blast furnace, yet no explosion took place.

I mentioned in my last communication to the Society, that a flame confined in a cylinder of very fine wire-gauze did not explode a mixture of oxygen and hydrogen, but that the gases burnt in it with great vivacity. I have repeated this experiment in nearly a pint of the most explosive mixture of the two gases: they burnt violently within the cylinder; but, though the upper part became nearly white hot, yet no explosion was communicated, and it was necessary to withdraw the cylinder to prevent the brass wire from being melted.

These results are best explained by considering the nature of the flame of combustible bodies, which, in all cases, must be considered as the combustion of an *explosive mixture* of inflammable gas, or vapour and air; for it cannot be regarded as a mere combustion at the surface of contact of the inflammable matter: and the fact is proved by holding a taper or a piece of burning phosphorus within a large flame made by the combustion of alcohol, the flame of the candle or of the phosphorus will appear in the centre of the other flame, proving that there is oxygen even in its interior part.

The heat communicated by flame must depend upon its mass; this is shown by the fact that the top of a slender cylinder of wire-gauze hardly ever becomes dull red in the experiment on an explosive mixture, whilst in a larger cylinder, made of the same material, the central part of the top soon becomes bright red. A large quantity of cold air thrown upon a small flame, lowers its heat beyond the explosive point, and in extinguishing a flame by blowing upon it, the effect is probably principally produced

produced by this cause, assisted by a dilution of the explosive mixture.

If a piece of wire-gauze sieve is held over a flame of a lamp cr of coal gas, it prevents the flame from passing it, and the phænomenon is precisely similar to that exhibited by the wire-gauze cylinders; the air passing through is found very hot, for it will convert paper into charcoal; and it is an explosive mixture, for it will inflame if a lighted taper is presented to it ; but it is cooled below the explosive point by passing through wire even red-hot, and by being mixed with a considerable quantity of air comparatively cold. The real temperature of visible flame is perhaps as high as any we are acquainted with. Mr. Tennant was in the habit of showing an experiment, which demonstrates the intensity of its heat. He used to fuse a small filament of platinum in the flame of a common candle ; and it is proved by many facts, that a stream of air may be made to render a metallic body white hot, yet not be itself luminous.

A considerable mass of heated metal is required to inflame even coal gas, or the contact of the same mixture with an extensive heated surface. An iron wire of $\frac{1}{20}$ of an inch and eight inches long, red hot, when held perpendicularly in a stream of coal gas, did not inflame it, nor did a short wire of one sixth of an inch produce the effect held horizontally; but wire of the same size, when six inches of it were red hot, and when it was held perpendicularly in a bottle, containing an explosive mixture, so that heat was successively communicated to portions of the gas, produced its explosion.

A certain degree of mechanical force which rapidly throws portions of cold explosive mixture upon flame, prevents explosions at the point of contact : thus on pressing an explosive mixture of coal gas from a syringe, or a gum elastic bottle, it burns only at some distance from the aperture from which it is disengaged.

Taking all these circumstances into account, there appears no difficulty in explaining the combustion of explosive mixtures within and not without the cylinders ; for a current is established from below upwards, and the hottest part of the cylinder is where the results of combustion, the water, carbonic acid, or azote, which are not inflammable, pass out. The gas which enters is not sufficiently heated on the outside of the wire, to be exploded; and as the gases are no where confined, there can be no mechanical force pressing currents of flame towards the same point.

It will be needless to enter into further illustrations of the theoretical part of the subject : and I shall conclude this paper by giving what I am sure will be gratifying to the Society, that the

the cylinder lamps have been tried in two of the most dangerous mines near Newcastle, with perfect success; and from the communications I have had from the collieries, there is every reason to believe that they will be immediately adopted in all the mines in that neighbourhood, where there is any danger from fire-damp.

VIII. *Account of a Calculus voided by a Female.*

To Mr. Tilloch.

SIR, — A FEW weeks since I was requested by a respectable surgeon to examine a fragment of calculus voided by a female patient of his. The fragment weighed about three grains and a half, had somewhat of a rhomboidal figure; was evidently convex on the one side, and concave on the other. The convex surface was considerably nodulated, but the concave was smooth. When viewed with a magnifier, it showed distinct marks of stratification of alternate layers of a grayish and dirty yellow coloured substance. On being heated to redness before the blowpipe it lost nearly ·3 of its weight; that is, after it had been kept in the heat of boiling water for some time: by urging the heat still further it fell to powder, lost its former colour, and gained a slight tinge of red, losing more than ·1 more of its weight.

I was then induced to try the effect of acids on this substance, and accordingly took a portion of it which had been treated as above, and found it to dissolve in muriatic acid without effervescence, leaving but the smallest possible quantity, which I conceived to be animal matter. I then took a portion of the substance as it was voided, which dissolved also in muriatic acid, but with considerable effervescence. I was at first unwilling to attribute the effervescence to carbonic acid; but upon examination by letting up lime-water into the gas in p'test tube standing over mercury, I was convinced of its being the case. The solutions were next examined, and were found to contain lime, phosphoric acid, and iron. Therefore the calculus is composed of carbonate and phosphate of lime, and oxide of iron, with a very minute portion of animal matter; for when the residuum, which was found to be insoluble in muriatic acid, was separated and heated to redness on a slip of platina, it exhibited distinctly the peculiar smell of burnt feathers or other animal substances*.

I am not exactly aware, whether the carbonate of lime has

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* From the figure of the fragment, as well as that which I have received, it has evidently been detached from a nucleus apparently 8-10ths of an inch in diameter.

hitherto.

hitherto been found to exist in human urinary concretions, at any rate, in the large proportion it seems to be in this; yet I have some faint recollection of having heard or read of the same thing having been noticed by Vauquelin (I believe). You must pardon me if I am incorrect.

Since I examined the above, I have received another portion; a more correct analysis of which I shall give you for insertion in your next number.

I remain, dear sir,

Very truly yours,

76, Drury Lane, July 13, 1816. JOHN THOMAS COOPER.

IX. *On Meteoric Stones.* *By the Rev.* T. DRUMMOND.

To Mr. Tilloch.

SIR, — UNWILLING to trespass on your pages by a long letter, I forbore to notice in my last, that it appears on the authority of Pliny, that meteoric stones or aërolites were supposed to have fallen from the body of the sun; your pages record that some modern philosophers have imagined them to have been projected from the moon. I will not trouble you at this time with any arguments in support of an opinion, that they are generated in the regions between our earth and the nearest planets; but since I do not remember that any of your correspondents have noticed the circumstance, I wish to mention that the same author records a report that Anaxagoras Clazomenius had predicted the fall of a meteoric stone, and that the prediction was verified. [Lib. ii. c. 58.]

If, sir, we admit the report to have been correct, may we not infer that astronomical calculations and a regard to planetary influences must have led the philosopher to deduce his inference?

If it be merely to gratify curiosity, I hope your readers in the different parts of the civilized world will transmit to you an accurate statement of the precise time in which the phænomena occur in future. Our pride in modern science must be humbled by every indication of the attainments of the ancients, unless we can *refute,* or *equal,* or *excel* them.

Yours respectfully,

Norwich, Gray Friars' Priory, T. DRUMMOND.
July 6, 1816.

X. *On*

X. *On the Accumulation of Heat by Friction.*

To Mr. Tilloch.

Sir, — The principal arguments that have been advanced against the existence of a material fluid of heat, are those derived from the experiments on the generation of heat by friction: from those experiments it is inferred, that the quantity of heat contained in a given piece of metal is inexhaustible. No direct experiment, however, has been made in support of this opinion ; but because a piece of metal connected with and surrounded by conductors, continued to give out heat as long as friction was applied, the author of the experiments concludes, " that any thing which any *insulated* body, or system of bodies, can continue to furnish *without limitation*, cannot possibly be a *material substance*."

If heat be a material fluid, the effect of force on a body containing it would be similar to the effect of force on a body containing any other fluid diffused through its pores in a similar manner. Water being a fluid which in many instances produces effects similar to those produced by heat, it appears best adapted to illustrate the generation of heat by friction.

I procured a piece of light and porous wood, three inches in length, two in width, and one in thickness; and having immersed it in water till it was saturated, I fixed it firmly over a vessel filled with water, the lower end being about half an inch below the surface of the water, and then moved a piece of hard wood backwards and forwards on the upper end, with a considerable degree of pressure. I thus found that water could be raised through the pores of wood by friction. The process is easily understood : the rubber, or piece of hard wood, as it is moved along, presses the water out of the pores, and closes them, driving the water which is pressed out before it ; but when the rubber has passed over those pores, the water from below rushes into them to restore the equilibrium.

The action of the blunt borer in Count Rumford's experiments appears to have produced a similar kind of effect ; the heat having been forced out of the pores of the metal by the borer, its place would be supplied by the heat from the adjacent parts. Gun-metal being a good conductor, the neck which connected the cylinder with the cannon would be capable of giving passage to all the heat that was accumulated from the cannon, and the other conductors with which it was connected.

Count Rumford considered it improbable that the heat could have been supplied by means of the small neck of metal, because heat was given out by it during the whole of the experiments.

ment; but one body may give out heat to another of a lower temperature, and yet be in the act of conducting heat.

If some imperfect conductor could have been substituted for the neck of metal, a difference in the quantity of heat generated, in a given time, would no doubt have been the consequence; but, unfortunately, this mode of varying the experiment did not occur to Count Rumford.

I am, sir, yours, &c.

X.

XI. *On the Ventilation of Mines.* By JAMES WATT, *M.D. of Glasgow.*

To Mr. Tilloch.

SIR, — THE late success of science, in obviating the danger from deleterious gases in mines, must be grateful alike to the philosopher and philanthropist. If you judge the following ideas conducive to the completion of a design so important, your insertion of them will much oblige the writer.

The merit of Sir Humphry Davy's lamp seems to justify its high character. Previous to this invention, the existence of the deadly explosive gas was detected in most cases only by the sad experience of its destructive effects; now, the danger may be safely detected and easily avoided. This lamp, however, does not prevent the production of the gas, nor render the workings safe where it greatly abounds. Nor does it prevent the danger of explosion in other cases, as when a fire is used, at the bottom of the upcast pit, to promote the ventilation. Besides, this lamp is not calculated to clear the mine of the *carbonic acid gas,* which, though detected with less danger than the other, is also deleterious to the workmen. Indeed, the principal merit of Davy's invention is, that it enables the miner to detect, by his light, the explosive gas, with equal safety and certainty as he formerly discovered the presence of the other. The most proper use of the discovery in both cases will be the same,—to make his escape, with all practicable expedition, till by the aid of ventilation the enemy be dislodged.

It is evident then, that though a complete method of ventilation would supersede the use and necessity of all such contrivances as Davy's lamp, however ingenious and sublime; yet no such invention could supersede the necessity of ventilation. To find a plan of ventilation, as practicable and as effectual as possible, is therefore an object of high importance.

In mines which are furnished with several shafts, a degree of ventilation is for the most part insured by the different temperature of the atmosphere above and in the interior of the mine.

In

In the heat of summer the current is likely to descend by the shaft whose top stands highest, and to make its exit by the shaft whose top stands lowest. In the cold of winter this current will be reversed. When the temperature is equal above and below, and where the current is too feeble, it must be promoted by fires or other artificial means.

To direct this current through all parts of the workings, various contrivances have long been used. These seem to be all superseded by Mr. Ryan's plan described in your last number, and the plan of Mr. Menzies, which is somewhat similar.

The immediate object of both these plans is to obtain from all parts of the interior of the mine an uninterrupted ascent of the roof, to the *upcast* pit, through which the current ascends. If such an ascent can be obtained, the *explosive* gas will certainly be discharged by its own specific levity. But such an ascent in the roof of a mine is sometimes prevented or destroyed by depressions in the roof, and by large masses of the roof which fall, leaving cavities to be receptacles for large quantities of explosive gas.

Besides, these plans, at least that of Mr. Menzies, is not so well calculated to ensure the *discharge* of the *carbonic acid gas*, which is itself a great object in ventilation. This gas is specifically heavier than common air, and lies in the most depressed parts of the mine.

Inequalities in the roof or floor of the mine provide for the retention of one or other of these gases, even in spite of a current of air passing through the mine. This may be illustrated by a transparent arched tube, transmitting a current of coloured fluid or of air. Let an air bubble, or bead of air, occupy the highest part of the arch, while a stream of coloured fluid is forced through the tube. This air-bubble will be seen keeping its place, or, if pushed out of it, regaining it speedily. Again, let the arch be inverted, and let a bead of coloured fluid occupy its lowest part; while a stream of air is forced through the tube; the coloured fluid will always speedily regain its situation in the lowest part of the arch. In this illustration, the stream of fluid in the one case, and the stream of air in the other, represent the current of air ventilating the mine, while the bead of air in the highest part of the arch, and the bead of coloured fluid in the lowest part, represent masses of the two foul gases retaining their situation in cavities in the roof and floor of the mine. It is true that a forcible current of air will dislodge these portions of foul gas; but the ventilating current is often not sufficiently powerful for this purpose.

But though this ventilation were complete *when once established*, the danger from deleterious gases must be guarded against

during

during the operations, before the communication for the venti-
lating current can be effected, and before the mine and roof could
be subjected to the plans of Ryan or Menzies.

Even where the works are complete, a change in the tem-
perature of the atmosphere may occasion a change in the di-
rection of the current, so that what was the *upcast* shaft may be-
come the *downcast* shaft [x]. Against this case, I think, Mr. Men-
zies' plan makes no provision.

A stream of water falling down through one of the shafts is
frequently used as a means of ventilation. This expedient is
suited, I think, to the case where only carbonic acid gas is pre-
sent; and besides, as the water must be lifted afterwards, this
mode of ventilation is too expensive to be practicable, except
on extraordinary occasions.

It appears then that a mode of *ventilation by mechanical
means*, and completely under control, would be highly eligible
in a great variety of cases.

Ventilation by mechanical means must be effected by a blowing
apparatus of some sort. This apparatus must be applied, either
to *force* a quantity of air *into* the mine, by which the foul gases
may be displaced, or to *extract* these gases out of the mine,
while common air will replace them spontaneously. In either
case, a tube should extend from the blowing apparatus to that
part of the mine in which ventilation will be chiefly required and
most useful. The mode of ventilation by *extraction* seems most
eligible, for two reasons: 1st, The extracting tubes can be di-
rected to any spot in the roof or floor of the mine where the foul
gas is lodged, and this gas only needs to be removed ; but by
blowing into the part, a quantity of harmless or salutary air must
also be forced away. 2dly, By the mode of extraction by a tube,
the foul gas is kept in a state of separation in its progress through
the mine and up the shaft ; but if it were to be discharged by
blowing inward, it might come in contact with the lights in its
progress, and explode, or prove otherwise dangerous or dis-
agreeable.

The extracting or blowing tube would be the most expensive
part of this apparatus. It should be flexible, or otherwise fitted
to apply to and feed from either the roof or floor at pleasure ; but
it might be formed of the cheapest materials : and in works in
which there is a spare shaft, this shaft may be employed to
furnish part of the ventilating tube.

The best of blowing apparatus for ventilating mines is

evidently

rightly understand Ryan's system, can never happen where
the upcast and downcast shafts are, we believe, never
changed.—Edit.

evidently that which will, in a given time, *transmit* the greatest
quantity of air, with the least expenditure of power. *Force* of
blast, or *compression*, are in this case no object: mere *trans-
mission* is all that is required. The common bellows have been
proposed to extract from the mine, by a tube descending from
the valve into the mine*. But here much power would be
wasted in rendering the blast forcible, and time in discharging
it. An air-pump with a piston has been also proposed; but
against it the same objections are valid; beside the additional
waste of power in overcoming the friction of the piston. A large
vessel, or rather a number of vessels, inverted in a receptacle of
water, and alternately raised and lowered by an axis revolving,
and furnished with cranks, or otherwise producing its effect,
would form a powerful blowing apparatus.

But the principle of the FAN or FANNER,—the WINNOWING
APPARATUS, seems free from all objections, and possesses the
greatest advantages. It transmits a great quantity of air. Its
movement is rotative, and hence it can be attached to the gin
or gig employed in raising the minerals, and even placed on the
same axis, so that the expense of construction will be trifling.
It acts by a centrifugal principle, and hence its powers of trans-
mitting air can be augmented to any assignable degree. It is
only needful to increase the diameter of the circle and the power,
and to make the entrance and exit for the air of suitable capa-
city. It can feed either from one end of the axis, or from both;
hence, either end may be inclosed, or tubes may extend from
both ends to different parts of the mine. The fan, as has been
said, may be placed on the same axis as the gin or gig, above
ground, and wrought by the same power. It may be placed in
any convenient situation below, and wrought by the power of
men, horses, asses or oxen. It may stand vertically or horizon-
tally, and it may *feed* by *numerous* tubes extending to either
end of its axis, from different parts of the roof or floor of the
mine, while it discharges into a spare shaft, or into the lower
end of the shaft employed for lifting the water, by *one* large tube
extending from the circumference of the box containing the
fan. In this way the expense of a tube would be saved by using
this shaft; for the explosive gas, at least, being introduced into
the bottom of the shaft, would ascend by its own specific levity.
The fan, thus employed, might obviate all danger both while the
works are forming and afterward: and it is
the interruption of ventilation from inequa
floor of the mine. It is even suited to su

* As far as I know, this idea was first suggested by a
lecturer on natural philosophy, who has been blind fro
hood.—J. W.

and expense of forming more shafts than one to a mine ; at least, for the purpose of ventilation ; for this might be effected by means of a tube carried down in one side or angle of a single shaft, and extending to the various parts of the excavation below ; and in extensive mines, several fans might be used with combined effect.

Mr. Editor, I am aware that the fan has been used to *blow* down a shaft a short way, but I am not aware that it has been employed in the above mode for *extracting* the foul gases from mines. Nor do I know that the vessels inverted in water have been used or proposed as a blowing apparatus. The powers and capabilities of the fan will appear to any one who has seen Mr. Sadler inflating his exhibition balloon.

The above ideas, on employing the fan, were lately read to the Glasgow Philosophical Society at two of their weekly meetings. The members present recommended that they should be given to the public through some of the scientific journals.

I am, sir,

Your obedient servant,

Glasgow, 16th July 1816.   JAMES WATT, M.D.

XII. *On Errors in the Nautical Almanac, &c.*

To Mr. Tilloch.

SIR, — MUCH has been lately said respecting the comparative merits of the Nautical Almanac, and the *Connaissance des Tems :* and attempts have been made to show that the computers of the latter work have borrowed very considerably from the former. There are two facts, however, which I have recently discovered, that will (I think) evidently show that the computations in those works are carried on independently of each other. These facts relate to Jupiter's satellites.

The *configurations* of those satellites, as set down in the Nautical Almanac for the last month (June), are almost all of them wrong ; as I found from actual observation, and as may be readily proved by calculation : many of them likewise are incorrectly stated for the ensuing month of August. In the *Connaissance des Tems,* however, the positions are truly stated.

On the other hand, the *eclipses* of those satellites are all correctly computed in the Nautical Almanac : but, in the *Connaissance des Tems* the eclipses of the *first* satellite are set down very incorrectly ; there being a constant error, which sometimes amounts to *six minutes of time,* either in excess or defect. This mistake runs through the whole of the years 1815, 1816 and
1817;

1817; and has arisen (I presume) from an error in Lalande's Tables at the end of the first volume of his Astronomy, p. 250, where the Argument C is erroneously stated 3000 too great, in the mean conjunctions from 1812 to 1820; an error which has been carefully avoided by Mr. Vince in his edition.

Whilst I am upon the subject of the Nautical Almanac, may I be allowed to ask whether any of your readers can inform me why the very excellent and useful preface which usually accompanies that work, has been omitted by the present Astronomer Royal, in the publications for the years 1817 and 1818. The tables from which that work is computed have been successively improved; and an account of the same, together with a statement of the tables used by the computers for the time being, has been hitherto preserved in the preface above alluded to. But a person who may now be desirous of ascertaining the correctness of any of the calculations, would be at a loss to know to what tables he should refer. This is the more to be regretted, as it is notorious that there is a considerable variance with corresponding calculations in other similar works.

For example, the times of new and full moon, as given in the Nautical Almanac, and in the *Connaissance des Tems*, for the present year, differ frequently (after allowing for the difference of meridians) many minutes, and oftentimes many *hours* from each other. This inequality in the results must, however, be attributed to some other source than the use of a different set of tables; all of which boast of considerable accuracy: yet, as it shows the necessity of a computation from the tables themselves, when any nice calculation is required, the knowledge of the identical tables made use of in the formation of the ephemeris, might lead to a detection of the error; as we have already seen in the case of Jupiter's satellites.

I shall not, however, intrude further on your indulgence, at present; my object being merely to call the attention of your readers (who may have more leisure) to an investigation of the subject: and I shall be happy if it be the means of correcting any errors which at present exist, or which in future may be likely to occur in those truly valuable works.

I am, sir,

Your obedient servant,

July 21, 1816.   ASTRONOMICUS.

XIII. *Experiments with Sir* H. DAVY's *Safe-Lamp.* *By*
Dr. HAMEL, *of St. Petersburgh.*

SOME time ago I had an opportunity of trying Sir Humphry
Davy's lamp in a coal-mine near Holywell in Flintshire. I de-
scended (along with Mess. William and Edward Roscoe) the pit
of Deebank colliery, 140 yards deep, and then proceeded hori-
zontally in one of the metal-drifts, where in one place the in-
flammable gas was bubbling with considerable force through the
water, covering the bottom of the mine. The ventilation here
being so complete as to prevent any danger from explosion, I
kindled the gas with a common candle. It continued burning
with a flame about 1½ foot long. Sir Humphry Davy's lamp
held in the same current would not set fire to it. We now went
to a place near the end of the working, where in the roof of the
mine there is a considerable excavation constantly filled with
carburetted hydrogen, issuing from a fissure in the roof on that
spot. Holding the safety-lamp in the lower part of this exca-
vation, where the inflammable gas is always mixed with atmo-
spheric air, a succession of slight explosions took place in the
inside of the lamp; but when raised into purer inflammable gas,
the whole of the cylinder was filled with a faint blueish flame,
through which that of the wick was distinctly visible. On lifting
it still higher into the purest carburetted hydrogen, the lamp
appeared extinguished, but rekindled spontaneously when in-
stantly withdrawn into atmospheric air.

Having convinced myself that the lamp would not set fire to
the gas (and having been breathing the same for some time, to
try its effects when taken into the lungs), we approached it with
a common candle tied to a long stick. The gas took fire with
considerable explosion, the lowermost stratum being mixed with
atmospheric air, and the remainder continued burning for some
time, filling three-fourths of the mine with an undulating blaze.
The appearance was awful, and gave me some notion of the man-
ner in which those unfortunate persons perish, who meet with
their death from accidents of this kind.

The lamp with which we made the experiment had a cylinder
of *brass*-wire gauze. It had become very hot during our trials
with it, and I think the flame was greener than is common to
carburetted hydrogen from coal-mines. I should suppose brass
or copper wire would not stand so long as iron-wire gauze.
Mr. Buddle writes me " that he has had several lamps with *iron-*
gauze cylinders for three months in daily use, without being in
the slightest degree impaired although they have been frequently
red hot for a considerable length of time." The chief doubts
remaining in my mind with regard to the complete safety of the
lamp,

lamp, were, that particles of coal, which generally fly about where
the men are working, might stick in the meshes of the gauze,
and by giving out a flame might kindle the gas.

I had an idea, that by putting over the gauze cylinder a se-
cond one of glass or gauze, this danger might be avoided; but
on mentioning my doubts to Sir Humphry Davy, he showed me
some experiments in the laboratory of the Royal Institution, by
which it appeared that coal-dust, even when laid on the top of
the lamp and becoming red hot, or when blown through the
gauze cylinder, whilst filled with the flame of burning gas, would
not inflame the surrounding explosive mixture. Sir H. Davy's
discovery of the property of wire-gauze is great: it has rendered
philosophy triumphant over an evil that had long baffled the
united efforts of the man of science and the philanthropist.

XIV. *Account of the Method of preparing or smoking Beef at
Hamburgh. By M.* PIERARD, *Captain of Engineers*[*].

HAMBURGH or smoked beef is in general estimation through-
out Europe; not only is it most agreeable to the taste, but it
has the property of being preserved a much longer time than
beef preserved in any other way.

The preparations necessary, are in the first place a salting-
house, some trays or tubs proportioned to the size of the pieces
of meat, and a drying-room in which they are to be placed when
properly salted.

In the salting-house is a kind of table made of oak, on which
the tubs are placed, and each has a coverlid, with a handle to
it. They are besides most carefully hooped, to prevent any waste
of brine. The salting-house is generally a cellar, on account of
the temperature being cooler.

The drying-place is generally part of a barn, into which en-
ters the flue from a small chimney. To the roof are affixed two
or three wooden beams or frames from which the meat is sus-
pended by hooks. The height of the drying-room above the
fire intended to give the necessary smoke, is generally from twelve
to sixteen metres, in order that, during its ascension into the
flue, the smoke may be freed from a great part of the substances
which constitute the soot, and thereby less bitterness be commu-
nicated to the meat which is to be dried.

In order to regulate the dispersion of the smoke over the
meat, a trap or to-fall is fitted to the aperture, which serves as
an outlet for the smoke; on the two opposite sides of the drying-
room

room apertures are made about thirty centimetres diameter: and at one end, one or two other apertures of fifty centimetres in breadth by seventy in height are made, which may be closed at pleasure by means of a shutter.

In large establishments, where not only pieces of beef of all sizes are smoked, but also hams, sausages and tongues, the drying- or smoking-room is erected over the salting-room, and the smoke is introduced through apertures made in the floor or in the sides, and which apertures may be opened and closed according to the quantity of smoke wanted.

The method generally pursued, in order to prepare smoked beef, consists in first salting the meat before subjecting it to the action of the smoke in the drying-room. After having chosen the pieces of beef which contain the fewest bones, they are allowed to remain untouched for two or three days, that the meat may become more tender, taking care to place them in a very cool place, not humid, and defended from the rays of the sun. Care is taken not to employ too lean meat, because it takes salt badly, and the larger pieces of fat are removed, because they on the other hand are prejudicial. Pieces which weigh from eight to twelve kilogrammes are preferable to smaller, which contain always more or less bone, notwithstanding the operation of taking out the bones to which they are subjected. When they smoke pieces of beef which weigh from four to eight kilogrammes, it is with a view to use them soon afterwards, and in this case they are smoked much less than when they are intended to be kept longer.

Winter is the period when the greatest quantities of smoked beef are prepared, because putrefaction is less to be guarded against at this season, and at the same time the meat is always most tender.

Old salt (clean muriate of soda) is regarded as the best, and it is used grated or pounded. New salt being generally deli-quescent is less adapted for salting, because it communicates a bad taste to the meat, renders its colour dull, and does not give it the consistency requisite to its preservation.

In order to salt meat, the pieces are placed on the oak table before mentioned, when salt is strewed over them, and they are afterwards rubbed on all sides with a flat stone, in order that they may impregnate the more. This operation is repeated until the meat absorbs salt no longer. There is no fear of oversalting; as it can only absorb a certain quantity of salt, and it is easy to freshen it by steeping it in water before using it. When this operation is terminated, the beef is heaped up in layers in a tub, at the bottom of which some brine has been put. This brine is prepared by boiling salt with seven or eight times its weight of

water,

water, or simply water strongly impregnated with salt. The whole is concluded with a strong layer of salt, and the pieces are covered with a lid, on which a weight is placed sufficient to plunge them in the brine.

In about three weeks, more or less, according to the bulk of the pieces, salt, and the time which it is wished to preserve them, they are withdrawn from the tubs: they are suffered to drip, and are then carried to the drying-room, where they generally remain fifteen days, or three weeks, to receive during that time the action of the smoke of a fire, which is kept up with only three or four pieces of oak chips, very dry. It has been remarked, that the smoke of resinous woods makes the meat contract a very disagreeable taste, and that those pieces which remained longest in the drying-room exposed to a feeble smoke were capable of being kept longer, because a more intimate combination took place between the smoke and the constituent principles of the meat.

When it is wanted to produce what is call *scarlet* smoked beef, the meat is only left seven or eight days in the tubs before drying it, or rather care is taken to rub it with a mixture of three parts of common salt and one of saltpetre; but if the latter gives a colour to the meat, it also renders it harder.

The beef when smoked is preserved in a dry and well aired place, and it is exported by placing it in layers in well-joined chests, and by filling with new ashes or chaff the vacancies between each layer.

The beef is generally dressed with vegetables, such as cabbages, potatoes, or carrots, after having been first washed in warm water, and steeped afterwards for twenty-four hours in cold water to freshen it: aromatics are sometimes thrown into the dish to disguise the smoky taste, which does not please all palates.

XV. *Thirteenth Report of the Commissioners for making and maintaining the Caledonian Canal. Dated May 17, 1816.*

THE works of the Caledonian canal have now advanced to such a state, that in every part of the line it is become necessary to calculate the application of labour and machinery, so that no obstacle may singly retard the completion of the canal, when the rest of it shall have become navigable throughout; and it is for the consideration of parliament, whether it may not be a real œconomy to augment the annual grant for the two next years, rather than continue it for three years at the usual rate of fifty thousand pounds per annum. Not only will any delay retard the receipt

receipt of tolls, which, according to the best information we have been able to procure, may probably produce forty thousand pounds per annum, when the benefit of the canal passage shall have become thoroughly understood ; but even the present loss, resulting from delay, is not inconsiderable, because the number of labourers and workmen is diminished, while the expenditure on management and superintendence remains unaltered. The degree in which this disproportion takes place, may be estimated by comparing the number of labourers and workmen now employed, with the highest average number in any former year ; and the average number in the last twelvemonth, having been four hundred and ninety, shows a decrease of four hundred and sixty-nine in nine hundred and fifty-nine, or almost half. The largest number we ever employed was during the three months of June, July, and August 1811, averaging at fourteen hundred and four. Even this number was not felt as an inconvenience by our superintendents, not even at Corpach, where the extraordinary accumulation mostly existed, in consequence of a depression of trade at that time, and a consequent stagnation in all branches of employment at Glasgow.

A comparison of the sum of money then and now expended in labour, gives nearly the same result. In our ninth Report the sum appears to have been fifty thousand pounds, in our present Report thirty thousand ; which again will be much diminished in our next Report, unless a larger grant than usual shall be assigned to this service in the present session of parliament.

This will be understood, when we state, that after it became evident to us in the year 1814, that oak timber of proper dimensions for the posts and bars of lock gates, was no longer procurable on any terms, and we had thereupon determined to apply cast iron to that purpose,—no time was lost in employing skilful persons in forming models for this new application of what from its many and increasing uses in this country, may be called a British material ; and after proper experiments by the modellers and iron founders, they became able to furnish a supply from the iron-works at Butterley in Derbyshire, some part of which has already been used at Clachnacharry, and from Pontcysylte in Denbighshire, for the Corpach district of the canal, of which none is actually arrived, though a freight is now daily expected.

Thus the respective iron foundries being now fairly in action, can furnish any quantity as soon as required ; and the labour and time necessary for planking and hanging the gates, after the arrival of the cast-iron work materials, makes it desirable to us, for the regular progress and uniform completion of all the canal

works,

works, that a sum of eighteen thousand pounds should be appropriated to this expenditure on iron castings in the next ensuing twelvemonth from the date of this Report: nor will this be doubted when we state, that sixteen pairs of lock gates of this kind might at this time be hung in their respective places, if ready, no more than two pairs being as yet in action: these however serve to prove satisfactorily the facility of their motion, two men being able to open or to close them, although no water is in the lock to lighten their weight and pressure. We shall postpone any particular description of these newly invented lock gates, until their actual use shall have enabled us to state all the details.

Adding to the above eighteen thousand pounds, four thousand pounds for the current expenditure on timber and machinery of various kinds, and three thousand pounds for management and miscellaneous expenses, it will appear that no more than twenty-five thousand pounds will remain for expenditure on labour, and that we should be compelled to dismiss many of our experienced workmen, much to the detriment of the work, in case the quantity of labour should afterwards again be increased, as indeed could not fail to happen.

ESTIMATE. We have endeavoured to ascertain the total expenditure which will be necessary before the Caledonian canal can become navigable throughout; and we are of opinion, that the sum of one hundred and sixty or one hundred and seventy thousand pounds will be sufficient; and as we have twenty-five thousand pounds remaining of the last year's grant, two grants of seventy-five thousand pounds each, or three grants of fifty thousand pounds each, will amply cover the expenditure.

Being aware that our present estimate is not conformable to what might have been expected from our calculation founded on Mr. Telford's estimate of October 1813, we have called upon him to explain the difference, no change of prices having been alleged to have taken place since that time; and it appears, that to his estimate of two hundred and thirty-five thousand pounds, not including payments for land and for management, we added for these purposes eighteen thousand pounds, which is so short of the truth, that the payments for land and quarry rents will probably exceed that sum: but it is obvious, that we could only venture to state the amount of valuations, and that we could not control the opinion of a jury; much less could we foresee that the intervention of any jury would become necessary, the expense of which in itself amounted to more than a thousand pounds. In effect, twenty-seven thousand pounds would not have been too much to have added to Mr. Telford's estimate, considering that the expense of management from that time to that extent

extend to six years instead of three, at the rate of about fifteen hundred pounds per annum.

Mr. Telford has omitted to add the usual ten per cent. upon engineering estimates, which is more than usually allowable in a work of unexampled dimensions, and which would have amounted to twenty-three thousand pounds ; as also the expense of steam-boats for towing large vessels,—and of moorings to be laid down in the lakes, estimated together at ten thousand pounds [*]. Thus the estimate of October 1813 should have been two hundred and ninety-five thousand pounds ; since which time one hundred and twenty-four thousand pounds have been expended, leaving about one hundred and seventy thousand pounds as the largest supposable estimate, from which ten thousand pounds will be deducted, if we succeed in placing the locks at Fort Augustus in the bed of the river Oich. A large portion of the per centage is in reserve for this contingency, but the greater part may be deemed to have been consumed in the unexpected occurrence of hard ground and rock cutting at Strone and at Muirshearlich hereafter to be described in this Report) and in dredging ope-rations, of which no experience existed, and which were certainly under-estimated by Mr. Telford.

STATE OF THE WORKS. Before we enter into any particular description of the present state of the works along the whole line of the canal, from Inverness to Fort William, it will be con-venient to advert to certain general points of information con-nected therewith.

Our two superintendents have extended their care to the north and south ends of Loch Oich respectively; the deepening of the loch by dredging machinery will be assigned to the Clachna-charry establishment, because experience in that art has been acquired by them in the similar loch of Doughfour.

The communication between Clachnacharry and Fort Augustus is maintained by means of a sloop of sixty-five tons burthen, which habitually navigates Loch Ness ; and we take this occasion to observe, that her voyages with an adverse wind are usually made in about twenty-two hours; that she has not yet experienced any material detention from the weather, and that the squalls of

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[*] Mr. Telford's estimate, October 1813 - - £234,734

Land and damages	-	-	-	-	-	-	18,000
Management, six years	-	-	-	-	-	-	9,000
Per centage for contingencies	-	-	-	-	-	-	23,473
Steam-boats and moorings	-	-	-	-	-	-	10,000
				Total	-	-	295,207
Deduct expended since October 1813					-	-	124,000
Present estimate, May 1816	-	-	-	-	-	-	171,207

wind

wind supposed to be so frequent and so dangerous, have not been found to exist in any prejudicial degree.

A smaller sloop of forty tons burthen performs similar service in Loch Lochie with respect to the Corpach establishment, and has experienced no difficulty in navigating that lake ; a sloop still remains at each end of the canal, for the occasional carriage of a freight of stone or provender, and the same crews which navigate the sloops on the lakes are then borrowed for that purpose.

The circumstances attending the navigation of the Corpach sloop in Loch Eil, and the Linnhè loch, are worthy of notice, because those lochs differ in nothing from Loch Ness and Loch Lochie except in containing salt-water instead of fresh. Inspection of any map of Scotland will show that the Linnhè loch is in fact a continuation of the great valley which extends from Inverness to the Western Sea.

The Corpach sloop was built in the year 1804, and for ten years navigated these salt-water lakes without intermission, bringing free-stone from Cumbraes in the Firth of Clyde, and rubble-stone from Fassefern ; lime from Lismore, and provender from Appin. In all this variety of service and length of time, neither the sloop nor any one of her crew have sustained the least injury. Indeed the facility of navigating the Linnhè loch is fully established by this simple fact ;— that the freight and insurance of cargoes to Fort William from Glasgow or Ireland costs no more than to Oban or Tobermory, which are south of this Linnhè loch ; into which vessels of three or four hundred tons burthen frequently run for protection, and obtain it there with such certainty, that not one has been lost since the canal works commenced in the year 1803. Large vessels of the above burthen are now built in Loch Eil, an active and intelligent shipwright having very successfully formed an establishment close to the sea entrance of the Caledonian canal at Corpach.

At the other end of the canal, no difficulty of navigation was anticipated by those who apprehended danger and detention in the lakes ; and in fact the head of the Murray-frith (more properly called Loch Beauley) must be deemed a well protected harbour from Fort George to Clachnacharry.

From this statement we may venture to conclude that all former objections to navigation of the Caledonian canal have now been removed by experience ; and that the advantage derivable from avoiding the dangerous passage northward by Cape Wrath and the Orkneys, will suffer no subtraction in the short and expeditious passage which will be open to vessels of all sizes when the Caledonian canal is fully open, and in use.

The absence of alternate tides, and indeed of any perceptible current

current in the fresh-water lakes, might appear to threaten deten-
tion in calm weather; such weather however is not common in
a mountainous region; and were it otherwise, we are not with-
out expectation of giving motion to the largest vessels, by an
expedient which can hardly fail of success.   We here allude to
the new application of the steam-engine for propelling vessels
through the water; a practice which has been carried to such
extent on the Clyde and elsewhere, that we have been led to
consider how far any such machines might be rendered useful
for saving horse-labour in towing heavy vessels along the Cale-
donian canal, and for occasionally assisting them in the lakes.
We are not without hope that the various steam-engines which
we now possess, especially two of great power, may hereafter be
applied to this purpose with much advantage; and we shall cause
this subject to be carefully investigated without delay.

The weather during the last twelvemonth, especially the win-
ter half year, has been remarkably unfavourable, as indeed ap-
pears from the daily register of the winds and weather [inserted
in the Appendix, and which when it has answered its imme-
diate purpose of showing that the winds on the line of the canal
are as variable as elsewhere, may be applicable for the purposes
of general information.]   From the 12th October 1804 to the
present time, this register has been kept at Clachnacharry, at
Fort Augustus, and at Corpach, and may be relied on, especially
as to the first and last of these places, for the most perfect
fidelity, and as much precision as the subject admits.

We have not considered it to be necessary to repeat in the
Appendix to this Report, the map which was inserted in the Re-
port of last year; which may be referred to in reading the follow-
ing description of the present state of the works along the en-
tire line of the canal, as no alteration affecting the map has
taken place.

CLACHNACHARRY DISTRICT.   The present scene of operations
being much confined to the Middle District, our description of
the state of the works at the two ends of the canal will exhibit
little variation from the Report of last year.

Beginning as usual at the Inverness end of the canal, we have
to state that the entrance lock (commonly called the Sea-lock)
and its gates are in a perfect condition, and that the banks which
we advanced into the sea four hundred yards, in order to obtain
deep water for the position of this lock, are protected from the
action of the waves by a coating of refuse stone brought from the
neighbouring quarry; and as far as the experience of last winter
proves any thing, the safeguard is likely to be effectual.

Above the high-water mark the continuation of these banks
is of course no further necessary, and at a very small distance
was

was found a spot of indurated clay, on which the second lock is placed, and by means of it, the level of the canal becomes raised quite out of the reach of ordinary tides. The gates of these two locks are made entirely of wood, as being liable to the effects of the salt-water, and more subject than any others to be struck by vessels which enter from the sea-ward.

The temporary dam outside the entrance of the first lock has been partly removed, though not to the full depth hereafter necessary for large shipping, and the excavation of the earth between the first and second locks has proceeded to a corresponding level, so that the entrance of coasting vessels would experience no obstacle.

The Culvert, which, with a back drain, was found necessary for draining the low lands of Muirtown, has been finished, and discharges into the sea, halfway between the high and low-water marks.

The Muirtown Basin extends eastward from the Clachnacharry or second lock, terminating at the Muirtown wharf and the adjoining bridge. Here the temporary wooden bridge still continues in use; but the iron castings for completing those already prepared for the iron turn-bridge, have been shipped at Gainsborough, and probably have been landed at Clachnacharry before now; the motion of this bridge, and of all others on the Caledonian canal, will be horizontal, consitituting what is usually called a turn-bridge, as distinguished from a draw-bridge, which is raised vertically.

The four Muirtown locks being connected together, as well as with the bridge and wharf, will require no more than five pairs of gates, each pair consisting of two leaves or valves, as usual. Of these the upper pair is finished, and as being more exposed to concussions than the others, is wholly made of timber; this upper lock gate is hung in its place, and has been caulked and pitched. Two of the other gates below (both of the iron-framed kind) are also finished and hung in their respective places; one leaf of the fourth gate is set up and is now receiving its planking; the iron castings for the fifth and last lock-gate are arrived, and all the five Muirtown lock-gates will be ready for use before the end of July next.

From this place to the bridge at Bught, distant above a mile, the canal is finished; this bridge, like that at Muirtown, awaits the iron castings, which will furnish means for substituting an iron turn-bridge in place of the wooden bridge now in use. Half a mile southward from this bridge the canal takes a more westerly direction under the steep hill of Torvaine: the operation by which the river Ness was diverted from its former bed for the length

length of half a mile, to make room for the canal and the canal*
bank, has been described in our third Report; and the public
road has since been turned from the confined and dangerous
situation which it heretofore occupied between Torvaine and the
river Ness; it now passes behind the hill, thus avoiding danger,
and shortening the distance, at the expense of a moderate ac-
clivity: for these purposes the new road extends nearly to the
burn of Doughfour.

We shall not repeat our former Reports as to the operations
which have secured the course of the canal from Torvaine to the
regulating lock, a distance of nearly three miles, throughout
which the canal is complete, and awaits only the admission of
water to become navigable.

The foundations of the regulating lock were of necessity laid
close to the river Ness, whose ordinary level was about twenty
feet above the necessary excavations for the lock pit : this diffi-
cult task having been accomplished and the masonry afterwards
completed in July 1814, the lock gates ought to have been hung
long since; but by an unlucky mistake in freighting vessels from
Gainsborough, part of the cast-iron materials for the lower gate
is not arrived, though now daily expected. The upper gate is
in its place complete, and affords security against the occasional
floods of the river Ness, which might otherwise by breaking
through the dikes above, rush into the yet unfilled canal, very
much to its injury.

The process of deepening Loch Doughfour and the passage
which connects it with Loch Ness, is highly important, and was
contemplated with some anxiety until the successful operation of
our dredging machine had been ascertained, as mentioned in our
last Report. The entrance of the canal from Loch Doughfour
we described to have been then cleared to a sufficient depth for
small vessels, and since that the shallows near Castle Spiritual
have been the chief scene of operation; the result of which is
that ninety thousand tons of gravel have been lifted and re-
moved within the last twelvemonth, and thereby a passage of
nine feet depth is now open into Loch Ness. The most im-
provable river channel was found to be on the west side of the
Gravel island, so that it became necessary to remove a part of
the point of land on which the ruins of Castle Spiritual will con-
tinue to stand close to the brink of the deepened water.

A depth of nine or even of ten feet will appear very inadequate
for the passage of large shipping into Loch Ness; but not only
the channel will be more regularly deepened before the dredging
machine is removed to the Middle District, but the formation of
a dam across the river at the lower end of Loch Doughfour will
maintain

maintain that loch at a high level, and afford the twenty feet depth of water which is to be established throughout the Caledonian canal.

At present nothing hinders coasting vessels from navigating into Loch Ness from the sea except the want of the lower gate at the regulating lock, and two lock gates at Muirtown. Our own sloops, one of which is now on Loch Ness, one at Clachnacharry, will be the first to profit by the opportunity, and will convey materials to the works in the Middle District at a much less expense than at present; and no doubt coal and lime for the district of Strath-herrick will be imported largely, and landed at the pier of Farigag, constructed for this purpose at the termination of the Inverfarigag road.

MIDDLE DISTRICT. At the upper end of Loch-Ness exists the only considerable obstacle we have to encounter; for all the other works require merely labour applied to objects wherein from experience we expect no extraordinary difficulty to occur. But at Fort Augustus a situation is to be found for the chain of four locks, or rather for the lowest lock of the four; for that once finished and a drainage thereby established, the upper locks may be constructed with certainty of success. The situation in which our attempts have hitherto been exerted is at the small island about two hundred yards above the bridge, or twice that distance from Loch Ness. It was not difficult to turn the river course wholly to the north side of the island by means of proper dams; but when this had been effected, the island itself was found to be so penetrable by water that even a steam-engine pit could not be sunk without the greatest difficulty.

It was accomplished however to the requisite depth of twenty-eight feet, soon after the date of our last Report, and preparations for affixing the great steam-engine were forthwith put in progress: the necessary piling and platform was finished and the engine-house erected last autumn; and since the weather has permitted masonry-work to proceed in the present season, the two large boilers have been fixed, and the cylinder, working-beam and cisterns are now placed in their proper situations. This great steam-engine, whose cylinder is four feet in diameter, will begin to work immediately, that is before the end of June; and as its power is that which is inadequately* computed to be equal to that of thirty-six horses, it will lift and discharge an immense quantity of water; we hope sufficient to permit the lock-pit to be sunk, and the future masonry in it to proceed

* This manner of estimating the power of steam-engines is understood to express the *power* they are capable of exerting; but as they may be made to work throughout the twenty-four hours, they do much more work than the number of horses at which their power is estimated.

without

without molestation. Still we have another engine of twenty-horse power, which if necessary may be brought from Corpach, and in the last resort the locks may be placed on the rocky bank north-side of the river Oich. This event however may be contemplated as extremely improbable.

Until the precise situation of the locks at Fort Augustus can be determined, the course of the canal immediately connected with them must also remain unsettled ; for which reason no commencement of excavation has yet taken place in the Government land, which extends six hundred yards above the River island whereon the great steam-engine is erected, and by the south-side of which the lower lock will be placed. The four connected locks will occupy a space of two hundred and sixty yards in length.

Beyond the march or boundary between the Government land and the Lovat property, the canal has been carried on to completion for the space of four miles, excepting only the Cullachie lock, and the additional earth work which became necessary from the change of its situation described in our last Report. It is remarkable that the rock on which this lock is built, is found to extend no further than was necessary for the purpose. The masonry of the lock itself is nearly complete, and the walls of the recess below the lock are commenced, the lower platform having been bolted down to the solid rock.

The Glengarry property intervenes between Lovat's southern boundary and Loch Oich, and in this space, about one mile and an half in length, the canal is not commenced, the attention of our superintendent in this part of the line being at present occupied in preparing a dredging barge for clearing a passage through Loch Oich. This operation, though from our experience in Loch Doughfoun we may now pronounce it to be sure in effect, is slow in execution, and unless speedily commenced in Loch Oich, the navigation of the whole canal may perhaps be retarded by this obstacle when all others are removed. For this reason the carpenters began to work on the dredging barge in November last, and she is now ready for caulking. The dredging machine which is destined to float in this barge is in store at Bona, and will be brought up Loch Ness when necessary. We have reason to expect that this engine is on an improved construction in comparison with that now at work in Loch Doughfoun, advantage having been taken of the experience there acquired. Four discharging boats or punts, which must be in constant attendance on the dredging barge, are in preparation at the north end of Loch Oich.

At the south end of this loch, which is almost four miles in length, the excavation is commenced, and for the first seven hundred

hundred yards is as deep as the summer level of the water in Loch Oich; for the next five hundred yards the ground is opened, but the excavation is not so much advanced. At the termination of this twelve hundred yards, is the summit of the Great Valley, which is occupied by the Caledonian canal and by lakes from Inverness to Fort William. Here for three hundred yards in length the ground is not yet opened; but from thence to Loch Lochine, nearly a mile, there is no interval in the excavation, which in this space averages at about eleven feet in depth. In the deep cutting at the summit of the land, horse-labour will be required for removing the earth, and a wood-framed stable with a barn have been erected, sufficient to contain eight horses and a store of provender.

Between Loch Oich and Loch Lochie one-third part of the excavation is finished.

CORPACH DISTRICT. We have had occasion to state in our former Reports, that the level of Loch Lochie will be raised several feet when the canal at the head of the loch is ready for use, and the regulating lock which is to be placed there, has been completed, and its gates hung. The river Lochie, which issues from the lower end of the lake of the same name, will then be turned into the new bed prepared for it at Mucomer, and the regulating lock adjacent to the present bed of the river will serve to dam up the passage of water and raise the present level of the lake.

This lock is in a state ready to receive its gates; at a short distance below it, the south bank of the canal is placed in the bed of the river Lochie, which is made to take a new passage outside the canal bank; but the excavation necessary for this purpose at the confluence of the rivers Spean and Lochie, has not produced earth enough to finish the southern bank of the canal, which for the space of two hundred yards remains to be raised eight feet in height by means of earth brought across the canal from the high ground north of it; and this is now in operation. The outside of the canal bank which is washed by the river, required some protection, which has been afforded by a coating of rubble cut out of the bottom of the canal. The rocky nature of the ground was indeed the reason for placing the canal in the former bed of the river. From this place through the farms of East-Moy, West-Moy, and Eracht, two miles in length, the canal is completely finished, comprehending the bank drain for intercepting the mountain brooks, the inlet of the arches at Moy, the outlet, and the masonry abutments of Moy bridges.

The easy and easy operation of puddling, or rendering water-tight the banks of the canal over the great

now in progress; but the rocky ground at Strone is not yet all
removed, still remaining nine feet deep for the space of one hun-
dred and sixty yards in the bottom of the canal.   From Strone
to Muirshearlich, a full mile, the southern bank of the canal is
formed of earth brought from the deep cutting of the latter place,
which has been a very tedious and expensive operation, and will
not be finished until the close of the present season.   Last year
we had reason to form the same expectation; but the earth near
the bottom was unexpectedly found to be unusually hard, and
the weather has been extremely unfavourable for this kind of
work from the beginning of November almost to the present
time.   The action of the horses which remove the earth by
means of rail-ways, becomes circumscribed as the work draws
towards its close, and their number is consequently diminished
from thirty to twenty-three; they are the property of the con-
tractor, and maintained by him.

From the deep cutting at Muirshearlich through Shangan,
Upper and Lower Banavie, to the chain of eight locks, a space of
two miles, the canal is perfectly finished; as also from the chain
of locks to the sea entrance at Corpach, about a mile distant,
including therein the two Corpach locks and the bason between
them and the sea entrance.

The whole of the masonry of the five several aqueducts of Loy,
Muirshearlich, Shangan, Upper Banavie, and Lower Banavie,
with the road-ways under each of them, remain as before re-
ported, perfect and uninjured by floods or other accidents.   The
great chain of locks rising from Corpach Moss, and the two
Corpach locks, require only the addition of lock-gates for their
completion.   The sea-lock, with its gates, which are of wood,
is quite ready for use; but the coffer-dam which surrounds the
entrance from Loch Eil cannot be removed until the lower gate
of the Corpach locks is hung, the bottom of the platform there
being four feet lower than the level of low water in Loch Eil.
The iron materials for these gates are now daily expected, having
been prepared at Pontcysylte in Denbighshire, and embarked at
Chester.

It remains to be mentioned that in the Corpach district we
have caused to be planted at Upper Banavie, at the chain of
locks, and near Corpach, not less than a hundred thousand seed-
lings of fir and larch and thirteen thousand young trees in the
present spring; and we shall not fail to plant the entire banks
of the canal as fast as we acquire means of protecting the young
trees from injuries of every kind.

PURCHASE OF LAND.   We have much satisfaction in stating
that by the payment of eight thousand pounds with the interest
due thereupon, to Colonel Macdonell of Glengarry, in full for the
land

land purchased of him at the two ends of Loch Oich, our transactions relative to the purchase of land have been terminated.

The amount of rent to be paid by us for the Redcastle quarry is to be determined under the direction of the court of session by William Fraser Tytler, esq. sheriff depute of the county of Inverness.

Accounts. The amount of our expenditure to the end of April 1816, appears to have been five hundred and ninety-four thousand pounds; twenty-five thousand pounds in full of the grant of the last session of parliament is now payable to us at His Majesty's Exchequer.

The account inserted in the Appendix is similar to all our preceding accounts; and the series is in such form as to embrace an extensive classification, and always to show the expenditure of the preceding twelvemonth, as well as of the total expenditure to the present time.

Mr. Telford's accounts and vouchers have been examined and passed by our accountant to the 12th day of August last; every part of our expenditure which does not pass through Mr. Telford's hands, is paid by ourselves, and audited under the direction of the Lords Commissioners of the Treasury.

<div style="text-align:right">

CHARLES ABBOT.
N. VANSITTART.
W. GRANT.
CASTLEREAGH.
MELVILLE.
BINNING.
CHARLES GRANT.
I. H. BROWNE.

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May 17, 1816.

XVI. *A few additional practical Observations on the Wire-Gauze Safety-Lamps for Miners. With some Evidence of their Use. By Sir* HUMPHRY DAVY.

I HAVE already published an account of the researches which led me to discover that explosion and flame are incapable of passing through tubes and apertures, which are nevertheless very permeable to both light and air, and I have described several contrivances for lighting coal-mines, in which inflammable air is disengaged, all founded on the same principle of security.

The lamp which has been found most convenient for the miner, is that composed of a cylinder of strong wire-gauze fastened round the flame by a screw, and in which the wick is trimmed by a wire passing through a safe aperture.

Lamps of this construction have been commonly used in

most dangerous mines in England for nearly five months, during which time no accident has happened, and no inconvenience occurred.

Since I first published the account of the wire-gauze safe-lamp, I have made a number of experiments on flame, which have led to some new philosophical views of this curious and important subject, and to some practical results, which I hope will be useful to the miner: these last will form the subject of the following pages.

I find that double cylinders of wire-gauze, so arranged that the wires are parallel to each other, occasion very little loss of light, and very much diminish the heat when the fire damp alone is burning within the cylinder; so that with double cylinders I have never observed the wire-gauze to become red hot. The double cylinder lamp, therefore, is preferable to the single one, whenever it is necessary to preserve a light for a long time in a highly explosive atmosphere, and it has likewise twice the strength of the single cylinder lamp.

If objections, which have been made, probably by persons who had never seen the lamps, against the weakness of the wire, had really been valid, it would be easy to double the thickness of it, or to have treble, or even quadruple folds of wire, with bars perfectly parallel. Layers of wire-gauze, for instance, of 25, 26, and 27 apertures to the inch in the cylinder of the common size, when arranged with a little care, intercept very little more light than a single layer.

I have had two lamps made of cylinders of copper perforated with numerous small apertures; but they are more expensive than cylinders made of copper wire-gauze.

I have had a lamp made of double wire-gauze with an exterior copper chimney, which can be raised or lowered, so as to consume larger or smaller quantities of fire-damp. This lamp, I find, may be made to burn in a highly explosive atmosphere without producing considerable heat, for any length of time; and it offers a convenient form of a lamp for destroying the fire damp.

It does not appear, from Mr. Buddle's communications, that the iron wire rusts in common use: should this be found to be the case in certain mines, copper wire may be used; the additional expense of this material is trifling; and even wire plated with silver would not be so costly as to be an object in the price of the lamp. In the double cylinder lamp, the copper wire will never become red hot; and I have had lamps made in which the inner cylinder was of iron and the outer one of copper wire.

Whenever a single wire lamp is made to burn in a very explosive atmosphere, the heat soon arrives at its maximum, and then diminishes; and this, as well as the wires burning out, is shown to

be unfounded ; the carbonaceous matter produced from the decomposition of the oil, tends not only to prevent the oxidation of the metal, but likewise revives any oxide already formed ; and this coaly matter, when the fire-damp is burning in the lamp, chonks the upper apertures of the cylinder, and gradually diminishes the heat, by diminishing the quantity of gas consumed.

In my early inquiries on this subject, I thought that some vitreous incombustible composition might be used as a coating to prevent the rusting of the wire, and I made some experiments on this subject. I find that a fusible compound of boracic acid and the earth called baryta, answers the purpose of a coating very well ; and if in any case experience should prove that such an application is necessary, it may be easily made.

It is obvious, that as long as the principle of security (which is to admit no aperture in a lamp of more than $\frac{1}{40}$ of an inch square) is preserved, the construction may be almost infinitely varied, and the fire-damp may be burned throughout the whole of a cylinder, or inflamed at certain surfaces only, and the light transmitted from other surfaces through horn, or mica, or glass. I long ago had a lamp made of glass, in which the fire-damp burnt round the wick only, from a circular surface of wire-gauze, and which, in an explosive mixture, produced very little more heat than in the common atmosphere ; but the facility with which glass is broken, and horn and mica injured, prevents me from recommending any lamps in which these substances are used for the common purposes of the miner.

I have tried a chimney of brass capped with a double wire-gauze cylinder, having two-thirds of its circumference opposite the flame open, and covered with wire-gauze. This, in an explosive mixture, produced very little heat, though it gave a tolerable light, and by increasing the surfaces impermeable, or by diminishing those permeable to air, the heat may be reduced in any degree*.

Whatever construction is adopted, too much attention cannot be paid to the form of the screw by which the cylinder or chimney is fastened to the lamp ; it ought to be strong and deep, and to consist of at least three turns.

When the fire-damp is inflamed in the wire-gauze cylinders, coal dust thrown into the lamp burns with strong flashes and

* The Rev. John Hodgson, whose zeal and activity in promoting the objects of these researches I cannot praise too highly, has had a lamp made upon this plan, having a thick plate of glass in a metallic chimney opposite a wire-gauze feeder, in which the supply of air is regulated by means of a slider. I found a model which he sent me to answer very well ; the only objection is, the chance of the glass being broken by a fall, or by a drop of water when hot.

scintillations. It appears that the miners were at first alarmed by an effect of this kind produced by the dust naturally raised during the working of the coals. I have made a number of experiments on this subject; but though I have repeatedly thrown coal dust, powdered rosin, and witch meal, through lamps burning in more explosive mixtures than ever occur in coal mines, and though I have kept these substances floating in the explosive atmosphere, and heaped them upon the top of the lamp when it was red hot, yet I never could communicate explosion by means of them. Phosphorus or sulphur are the only substances which can produce explosion by being applied to the outside of the lamp; and sulphur, to produce the effect, must be applied in large quantities and blown upon by a current of fresh air.

It will be unnecessary to caution the workmen against heaping sulphur, or gunpowder, or pyrites, which afford sulphur by distillation, upon their lamps; and such dust from these substances as can float in the atmosphere cannot produce inflammation; for minute particles of ignited solid matter have no power of inflaming the fire-damp; and I have repeatedly blown fine coal dust mixed with minute quantities of the finest dust of gunpowder through the lamp burning in explosive mixtures without any communication of explosion: and supposing danger with respect to gunpowder, the steel-mill must be much more liable to inflame that substance than the wire-gauze lamp: and the double cylinder lamp offers perfect security against the inflammation of any matter that can ever exist in coal mines.

In adopting from 30 to 26 apertures to the inch (from 900 to 676 in the square inch) and wires of from $\frac{1}{50}$ to $\frac{1}{40}$ of an inch in thickness, even single lamps are secure in all atmospheres of fire-damp; and double cylinder lamps are perfectly safe under all circumstances even in atmospheres made explosive by coal gas, which, from the quantity of olefiant gas it contains, is much more inflammable than fire-damp. When indeed a strong current of coal gas is driven from a blow-pipe, so as to make wire-gauze of 676 apertures strongly red hot in the atmosphere, the flame from this pipe may be passed through it whilst it is strongly red hot; but this is owing to the power which wires strongly ignited possess of inflaming coal gas [*]; and they have no such effect on genuine fire-damp; and a stream of gas burning in the atmosphere acting on a small quantity of matter, is entirely different from an explosive mixture, which is uniform within the lamp.

[*] Olefiant gas contains twice as much charcoal as light carburetted hydrogen, and is more easily decomposed by heat; the density of its flame, as I shall on a future occasion, depends upon solid matter produced in the process of combustion, and which be... in circumstance holds good of all flames afford-

In a case where human life is concerned, and by which human happiness may be affected, I have thought it right to take notice of the most trivial and insignificant objections, even when they have arisen from ignorance, or have been prompted by malevolence; but I do not think it necessary to name the individuals by whom they have been made, for I would willingly consign to forgetfulness those who do not deserve to be remembered.

The evidence of the use of a practical discovery is of most value when it it is furnished by practical men. I shall therefore annex two communications, which I am sure will have due weight with all persons who are acquainted with the northern collieries; and my motive for publishing them is the hope of inducing the coal owners in other parts of the island to lose no time in adopting these simple methods of preserving their workmen from danger *. ——— II. D.

A Letter on the practical Application of the Wire-gauze Safe-lamp, from JOHN BUDDLE, *Esq. to Sir* H. DAVY.

Walls-end Colliery, Newcastle, 1st June, 1816.

SIR,—After having introduced your safety-lamp into general use in all the collieries under my direction, where inflammable air prevails; and after using them daily in every variety of explosive mixture for upwards of three months, I feel the highest possible gratification in stating to you, that they have answered to my entire satisfaction.

The safety of the lamps is so easily proved, by taking them into any part of a mine charged with fire-damp, and all the explosive gradations of that dangerous element, are so easily and satisfactorily ascertained by their application, as to strike the minds of the most prejudiced with the strongest conviction of their high utility; and our colliers have adopted them with the greatest eagerness.

In the practical application of the lamps, scarcely any difficulty has occurred. Those of the ordinary working size, when prepared with common cotton wick and the Greenland whale oil, burn during the collier's *shift*, or day's work of six hours, without requiring to be replenished; and the safety trimmer answers the purpose of cleaning, raising, and lowering the wick completely.

The only inconvenience experienced arises from the great quantity of dust, produced in some situations by working the coal, closing up the meshes of the wire gauze, and obscuring the light;

* Models of the double-cylinder lamp, which .... in all collieries where explosive mixtures are common, ... the single lamp, may be had of Mr. Newman ....don.

but the workmen very soon removed this inconvenience by the application of a small brush.

We have frequently used the lamps where the explosive mixture was so high as to heat the wire-gauze red hot ; but on examining a lamp which has been in constant use for three months, and occasionally subjected to this degree of heat, I cannot perceive that the gauze cylinder of iron wire is at all impaired. I have not, however, thought it prudent, in our present state of experience, to persist in using the lamps under such circumstances, because I have observed, that in such situations the particles of coal dust, floating in the air, fire at the gas burning within the cylinder, and fly off in small luminous sparks. This appearance, I must confess, alarmed me in the first instance ; but experience soon proved that it was not dangerous. As it is, however, possible that some other light combustible substance, capable of inflaming at a red heat, may occasionally float in the atmosphere of the mine, I have thought it prudent, for the present at least, to discontinue the use of the lamps where the gauze is subject to that degree of heat, especially if for a length of time at once.

Our colliers have found it most convenient to hang the stationary lamps from small wooden pedestals ; but on observing, that where the sides of the lamps have been suffered to come in contact with the pedestals, the wood is charred to a considerable depth by the heat of the lamps ; I have thought it right to use small iron pedestals instead of the wooden ones.

Beside the facilities afforded by this invention to the working of coal mines abounding in fire-damp, it has enabled the directors and superintendants to ascertain with the utmost precision and expedition, both the presence, the quantity, and the correct situation of the gas. Instead of creeping inch by inch with a candle, as is usual, along the galleries of a mine suspected to contain fire-damp, in order to ascertain its presence, we walk firmly on with the safe lamps, and with the utmost confidence prove the actual state of the mine. By observing attentively the several appearances upon the flame of the lamp, in an examination of this kind, the cause of accidents which have happened to the most experienced and cautious miners is completely developed ; and this has hitherto been, in a great measure, matter of mere conjecture.

When the discharge of inflammable air is regular, and the density of the atmosphere continues uniform, the firing point may be judged of, and approached with safety by a common candle. But when the discharge of inflammable air is irregular, or the atmosphere is in an unsettled state, a degree of uncertainty and danger attends the experiment of ascertaining the state of a mine.
                                                                    With

With the safe-lamp, however, it is reduced to the utmost certainty, the actual presence and position of the gas is not only ascertained with the greatest precision, but also every alteration of circumstance or position is distinctly perceived.

By placing a lamp near the spot where a quantity of inflammable air is issuing, and mixing with the circulating current of atmospherical air to the firing point, it will be seen that very remote causes frequently produce pulsations in the atmosphere of the mine, which occasion the gas to fire at *naked* lights ; thus showing clearly the instability of the element with which we have to deal, and the reason why so many explosions have occurred where lights have not approached the place where the gas was lodged within a considerable distance.

Objections have been made by some who have not had experience of the lamps, to the delicacy of the wire-gauze, under the apprehension that it may be very soon impaired by the flame within the cylinder. Of this, however, I have no reason to complain, as, after three months constant use, the gauze has not, in the hands of careful workmen, been perceptibly injured by the action of the flame ; but the outer top gauze of one or two of Newman's making has been worn through by the friction of the rivet* on the bottom of the swivel, to which the finger ring is fastened ; but this only happened to the lamps used by the *waste-men*, whose business it is to travel daily in the various avenues of the mines, for the purpose of keeping the passage for the current of air free from obstructions : nothing of the kind has happened to the stationary lamps used by the colliers.   In short, I do not apprehend that the gauze can be injured by any ordinary cause without being observed in time sufficient to prevent accidents ; and that we have no danger to apprehend, except from the gross negligence of some heedless individual, or an accident of a very unusual description occurring to injure the gauze.

I find that I have extended my letter to a greater length than I intended ; but I trust, Sir, that you will excuse me for having gone so much into detail, as I feel peculiar satisfaction in dwelling upon a subject which is of the utmost importance not only to the great cause of humanity, and to the mining interest of this country, but also to the commercial and manufacturing interests of the United Kingdom ; for I feel convinced that by the happy invention of the safe-lamp, large proportions of the coal mines of the empire will be rendered available, which otherwise might have remained inaccessible—at least without an invention of similar utility, it could not have been wrought without much loss of the mineral, and risk of life and capital.

* This rivet is now fixed. *Ed. Jl. IX.*

It is not necessary that I should enlarge upon the national advantages which must necessarily result from an invention calculated to prolong our supply of mineral coal, because I think them obvious to every reflecting mind ; but I cannot conclude without expressing my highest sentiments of admiration for those talents which have developed the properties and controlled the power of one of the most dangerous elements which human enterprise has hitherto had to encounter.

<div align="right">I have the honour to be, &c. &c.</div>

To Sir H. Davy.   JOHN BUDDLE.

<div align="center">

Extract of a Letter from Mr. PEILE *to Sir* H. DAVY.

</div>

<div align="right">Colliery Office, Whitehaven, 6th July, 1816.</div>

I TAKE the liberty of adding a further statement on your invaluable safe-lamps, in the Whitehaven collieries belonging to the earl of Lonsdale, since the first application of them in February last.

With us, the general use of the lamps in consequence of the good state of our ventilation is confined to leading workings, or trial drifts ; and in two of these, lately going on in one of the pits unusually infected with fire-damp, and which previously were lighted by means of steel-mills, we applied the lamps with great confidence and security.

In May last in these drifts an extraordinary discharge of fire-damp burst from the pavement of the mine, and the ventilation being at that time unavoidably obstructed, the atmosphere became so charged with fire-damp as to be nearly throughout an explosive mixture.   In this situation we derived the unspeakable benefit of light from the lamps, and, notwithstanding the explosive state of the mixture, with the most perfect safety.

In several other places in the collieries the lamps are used with the same confidence : yet the discharge of fire-damp being moderate, they are not much exposed to explosive mixtures.

In all the workings showing the least appearance of fire-damp, the miners are supplied with lamps, and are particularly cautioned to use them on first entering when beginning to work, where, being satisfied of security, they occasionally resort to candles afterwards.   This application of the lamp alone, is of the greatest utility, and prevents many slight explosions, and the miners from being burned ; besides superseding the necessity of depending on the judgement or discrimination of any individual to prove the existence of the fire-damp, as in the old method, by the candle flame.

From the repeated proofs made with the lamps, we cannot too strongly express our confidence in their security.

By experiment, a pint of oil, value six-pence, will about supply

<div align="right">ply</div>

ply a lamp for six days, the ordinary time of a man's working, so that they are cheaper than candles.

If my humble testimony can in any degree promote the speedy use of the lamp in other places, it will give me great pleasure.

<div align="right">I remain, &c.</div>

To Sir H. Davy.   JOHN PEILE.

XVII. *Extract of a Letter from Dr.* OLBERS, *F.R.S. of Bremen, respecting a New Comet; with an Account of a Work of Professor* BESSEL. *Communicated by a* CORRESPONDENT.

AFTER a long silence I profit by the permission which you once gave me to address you in German, not being able to express myself readily in English, though I read the language without difficulty. My present object is to request a favour of you, which may give you some trouble, but which I hope the importance of the subject will induce you to grant me :—it is, that you would undertake to collect subscriptions for a work of Professor Bessel, of Königsberg, of which the prospectus is inclosed. I am willing to hope that an undertaking so essential for the advancement of astronomy will be particularly encouraged in England, especially as it is entirely founded on the observations of your incomparable countryman Bradley. According to the present course of exchange, the price of the book will not exceed about a guinea and a half; and it will be printed as soon as a sufficient number of subscribers shall be found to afford a reasonable prospect of the repayment of the expenses.

I discovered a comet last year on the 6th of March, and gave immediate notice of the discovery to Dr. Herschel, as well as in the public papers. This comet was observed in Germany until the 25th of August; and it is very remarkable for the shortness of the period of its revolution. Several of our astronomers, and in particular Mr. Nicolai, now Director of the Observatory of Mankeim, have calculated its motions in an elliptical orbit; and the results of their calculations agree extremely well with each other; but none of them has exhibited greater diligence and address in the inquiry than Professor Bessel. These are his elements, determined for the 26th April 1815 :

Time of the perihelion, 1815 April 25·998674, mean time at Paris.
Longitude of the ascending node ...... 83° 2´ 33·63″
Inclination of the orbit .... 44° 29´ 54·59″
Distance of the perihelion from the node 65° 33´ 22·29″
Logarithm of the shortest distance .... 0·0838109
Eccentricity ... ... 0·98121·68
Greater semi-axis, or mean distance . 17·63383
Period ... ... 74·04913

The longitude of the ascending node is calculated from the mean place of the equinoctial point on the 1st Jan. 1815; and the inclination relates to the plane of the ecliptic at the same time. In this determination the perturbations derived from the action of the planets, during the time that the comet remained visible, have been taken into the calculation; and it is derived from a comparison of nearly 200 observations. According to the doctrine of chances, it is probable that the error of the time of revolution does not exceed ± ·27657 of a year, or about 101 days.

The indefatigable Bessel has also calculated the perturbations which this comet will undergo before its next return. According to its regular elements it ought to come again to the perihelion on the 14th May 1889; but, on account of the perturbations, this event will take place 824·51 days earlier, that is, on the 9th February 1887; consequently our successors may expect its return three or four months before or after the 9th February 1887.

If this comet should have been accurately observed in England, you will much oblige me by sending me the particulars of the observations. In France it has not been done with much precision; and we have only been able to obtain, in addition to the German observations, some which have been made in Russia and in the north of Italy.

In the first part of the Philosophical Transactions for 1814 is an excellent essay by your great mathematician Mr. Ivory, who is also so much esteemed in this country; it is entitled " On a New Method of deducing a first Approximation to the Orbit of a Comet." I had great pleasure in observing the striking resemblance between this method and that which I myself published some years ago, and which has since that time been much used on the Continent; and the coincidence was the more interesting, as Mr. Ivory obtained the same solution, from his algebraical investigation, which I had deduced from a more geometrical consideration of the problem. My method may easily have escaped Mr. Ivory's notice, as it was for a long time only to be found in German publications: at present he will have found it in Delambre's Astronomy, and perhaps also he may be acquainted with the new and very convenient form which Professor Gauss has given to it in the Göttingen Transactions. I send with this letter two copies of my little work; one I beg you to honour with a place in your library, the other to transmit to Mr. Ivory, with the assurance of my particular respect.

I am very desirous of being informed of Troughton's new circle at Greenwich, with which Mr. Pond now makes his observations, and in the place of a bird's quadrant with which the principal

part

part of Bradley's were made. Bessel finds the latitude of the quadrant, from 2000 observations of Bradley, exactly a second greater than Pond makes that of his circle; and it is natural to inquire whether a part of the difference may not depend on the different situation of the instruments.

Bremen, 15th June 1816. WILLIAM OLBERS.

Professor Bessel's work will be entitled *Fundamenta Astronomiæ pro Anno 1755, deducta ex Observationibus Viri incomparabilis* J. BRADLEY. It is the result of the labour of seven whole years, and contains a number of determinations of the utmost importance to astronomy. The extreme accuracy of Bradley's methods of observing, combined with the excellence of his instruments, made it possible for him to attain a degree of precision, which is of so much the more value, as half a century is now elapsed since the period to which they relate. All the elements which have any influence on astronomical observations are here deduced from Bradley's observations alone.

The sections of the work are these: I. Examination of the instruments, and of their corrections. II. Observations of the equinox. III. Latitude of Greenwich. IV. Refraction. V. Application of the latitude and refraction to the observations of the solstice and the equinox. VI. Examination of the old mural quadrant: law of its variation: new examination of right ascensions. VII. General and special tables of aberration and nutation. VIII. Register of the clock from 1750 to 1762. IX. Annual parallax of some of the fixed stars: constant multiplier for aberration. X. Catalogue of all the stars observed by Bradley; 3166 which are found in later catalogues, and 108 observed but once, and not elsewhere noticed; some of which may possibly have been planets, but the Georgian planet occurs but once among them. XI. Precession of the equinoxes. XII. Proper motion of the fixed stars. XIII. Comparison of the results with other observations.

The work is written in Latin, and will contain about 400 pages in folio: the price to subscribers will be ten dollars, conventional currency, to be paid on delivery. The only reason for soliciting a subscription is to contribute to the advancement of science. The author would readily have renounced all hopes of remuneration for his labours; but so little encouragement is given in Germany to works of the kind that no bookseller could be found to undertake the expense of the publication. A hundred subscribers will enable the author to meet these expenses, and their names will be published as benefactors to the science of astronomy.

* The Editor of the Philosophical Magazine has been requested

quested to receive the names of subscribers to this work, and
will forward them to Dr. Olbers's correspondent, who has under-
taken to be responsible for its delivery.

XVIII. *Notices respecting New Books.*

*Remarks on the Art of making Wine ; with Suggestions for the
Application of its Principles to the Improvement of domestic
Wines. By* John MacCulloch, *M.D.* Longman. pp.261.
12mo.

D*r.* MacCulloch's name as an author in the sciences of che-
mistry and mineralogy is deservedly in high estimation, and he
has added considerably to his reputation by the above publica-
tion. It is a practical treatise on a subject which is likely to be-
come daily of more interest in this country, and it has been an
object of peculiar attention with the Caledonian Horticultural So-
ciety, who have the merit of being perhaps the first public body
which has awarded premiums for the best specimens of British
wines.

Dr. MacCulloch's ideas were first thrown into the form of a
memoir for the Transactions of the above Society, and the en-
couragement they received in that shape induced him to ex-
tend his researches, and to compose a useful portable volume on
the subject. The author's treatise is quite practical : the im-
provements in modern chemistry have enabled him triumphantly
to explode the gross errors which had crept into most of our fa-
mily receipt-books for making wines. On the subject of the
absurd and deleterious practice of adding alcohol or brandy
to home-made and foreign wines, professedly to make them
keep longer ; but in reality to render them more palatable to the
depraved appetites of English wine-drinkers, the author is pecu-
liarly happy. Alluding to the undefinable light and quick fla-
vour so characteristic of the French wines, and so opposite to the
mawkish, dull and heavy taste of the wines of Spain and Portugal
(particularly that called Port), imported into this country, Dr.
MacCulloch ascribes the wretched taste and bad effects of the
latter to the admixture of brandy or spirits. We shall select
this part of the work, as being a specimen of its real value, and
as likely to be useful both to the consumers and makers of wines
in this country.

" This practice (that of mixing brandy or alcohol with wine),
universal in the wines of Spain, Portugal, and Sicily, which are
intended for the English market, has also been introduced into
our domestic wines, under the mistaken notion of preventing
                                                          them

them from turning sour, and with the idea that it enabled them to keep for a longer time. As this is a question of some importance, both as it regards the perfection and the œconomy of our domestic manufacture, I shall make no apology for entering somewhat largely into it.

" It had been long thought, from the vain attempts of chemists to separate the alcohol which is a constituent part of wine by other chemical processes than distillation, that this substance existed in it, either in a far different condition from that in which we know it when in its separate state, or that the intoxicating substance contained in wine was not alcohol. This opinion appears to have originated with Rouelle, who imagined that alcohol was not completely formed until the temperature was raised to the point of distillation. More lately the same doctrine was revived and promulgated by Fabbroni, in the Memoirs of the Florentine Academy. His opinion was founded on the following experiment. When alcohol is added to new wine, he observed that he could, by the introduction of subcarbonate of potash in sufficient quantity separate the added alcohol, while the spirit of the wine remained attached to it as before these additions, and could only be obtained by subjecting it to distillation. Hence he concluded, that the alcohol was formed by the action of heat on the elements of wine, or that it was a product of distillation.

" But the experiment was not attended with similar results in the hands of other chemists, unless in cases where the added alcohol bore a very considerable proportion to the wine, and it consequently left the question respecting the formation of alcohol in wine in the same state as before. I need not point out the laxity so apparent in Fabbroni's reasonings, as it would lead to discussions too minute for this essay. But the attention of other chemists has been excited towards the same object, and conclusions the reverse of his have been the consequence. If subacetate of lead be added to wine, and the clear liquor be then saturated with subcarbonate of potash, the alcohol will be separated. It also appears from the experiments of Gay Lussac that alcohol can be separated from wine by distillation at the temperature of 66, and indeed from the trials of Fabbroni himself, this separation was produced at 63. More recently by the aid of a vacuum the distillation has been effected at 56, a proof that alcohol is not produced by the action of the heat required for boiling wine or wash on the elements which these substances contain. It must therefore be considered as one of the elementary constituents of wine, and whatsoever phænomena it may therefore present with reagents or as a subject of chemical inquiry, must, as far as they may differ in different wines, arise from differences in its mode of combination with one or more of the

the other ingredients which enter into that compound fluid. Al‑
though therefore alcohol is ascertained to exist ready formed in
all wines, so much of the experiment of Fabbroni is still true as
to be an object of utility in the subject we are now examining;
while the conclusions to be drawn from it appear of importance
in explaining the different effects of simple wines and wines with
which brandy has been mixed. It is presuming much too far
on our chemical knowledge to imagine that we are acquainted
with the nature of alcohol. It is possibly subject to varieties of
composition, analogous to those which are found in the very va‑
riable substances included under carburetted hydrogen gas, and
which, although they have not been appreciated by chemical
actions, merely perhaps because the subject has been supposed
to be already understood, and the substance itself imagined to be
in all cases chemically identical, may be ascertained hereafter
by more refined experiments in the hands of acuter chemists, to
whom this subject is here pointed out as calling for investiga‑
tion. It is otherwise impossible to understand the differences
which appear in its effects on the nervous system when taken
into the stomach under its different forms. Where its specific
gravity, and consequently its imagined condition, is in different
specimens absolutely equal, yet these specimens, produced un‑
der different circumstances, operate on the nervous system in a
manner so totally different as to point out some radical differences
of which specific gravity is no criterion. The comparative ef‑
fects of new and of old rum, of equal *proof*, of Dutch gin and
of diluted alcohol of equal strength, are too well known to re‑
quire more than a bare mention. It has been supposed, for
want of better means of explaining these effects, that they were
due to the essential oil contained in the different varieties of
spirits used as beverage. But of these we know nothing. We
are sure that they are in very trifling quantity, since they cannot
be separated by water, except in the single case of gin, where a
foreign oil is purposely introduced. Neither are we acquainted
with any oils of this class whose qualities are deleterious, except
that of the bitter kernels, their analogous kindred laurel, and
a few other bitter vegetables, whose poisonous qualities besides
appear to be subject to no modifications; being, where not
deadly, almost unproductive of any effect. Moreover that spirit,
alcohol, from which by careful rectification the essential oil
seems to have been most carefully removed, is more injurious
than those which rum and brandy are known to contain it.

"Dilution does not remove the injurious effects of these noxious
spirits. When a spirit of any sort is mixed with water it requires
some time to affect the union of the two substances. They
unite and become combined. If the effect of the one kind of
　　　　　　　　　　　　　　　　　　　　　　　　　　　spirit,

spirit, although combined with water, is still different from that
of the other on the human body. They are both very different
from that of pure wine in which either chemical reagents or
distillation have ascertained precisely the same proportion of al-
cohol. In other words, the same quantity of alcohol applied to
the stomach under the form of natural wine, and in a state of
mere combination with water, is productive of effects on the
body, not only immediately but ultimately attended with consi-
derable differences. These are well known to physicians. They are
equally well known to those whose habits of observation either on
themselves or others have led them to compare the moral effects,
if we may so term them, produced by intoxication with different
wines, with champagne and claret, or with port and sherry, the
elevation of thought produced by the former, with the sedative
effects of the latter; or who have had an opportunity of wit-
nessing the stupidity produced by ale and the ferocity which re-
sults from intoxication with spirits. The nervous system is here
a test of differences which elude the ordinary resources of che-
mistry. Yet the reagents which have been applied to the in-
vestigation of these differences, although they have done little,
still show that some chemical distinctions may really exist. It
has been perhaps hastily said of Fabbroni's experiment, that it
was useless since it produced no consistent results. On the con-
trary it appears to be a test applicable to some of the least tena-
cious combinations of alcohol, and the censure unjustly passed
on it has originated in want of attention to the subject, and to
those delicate circumstances in the combinations of alcohol, on
which its various effects, as it exists in wine and other potable
liquors, depend. Could we discover an additional number of
such reagents differing in their various powers of separating the
different combinations under which it is found, I have little
doubt that chemical means would shortly illustrate, by corre-
sponding differences of effect, the different powers which these
beverages exert on the nervous system. The experiment of
Fabbroni is perfectly valid to a certain extent, and the causes of
the supposed irregular results appear to be abundantly obvious.
If alcohol be mixed with water in any proportion, it may be se-
parated by carbonat of potash. If it be mixed with wine in the
same manner, it is, with due attention and in particular circum-
stances, equally separable. But if an attempt be made to se-
parate the adventitious alcohol from those wines to which it has
been added by the manufacturer, the experiments will sometimes
succeed and sometimes fail. The cause is mentioned in various
parts of this essay, and is apparently this. If the water or
brandy be added before the fermentation of the wine, or at any
subsequent stage when that fermentation can be ...

excited, it then enters either entirely or partially into a more
perfect combination with the wine than that which it forms after
a mere admixture ; or a portion of it at least, proportioned to
the degree of fermentation which takes place after its addition,
becomes thus combined.   Here the test indicated by Fabbroni
fails, although reagents of higher powers are still capable of ef-
fecting the separation,   In all such cases the wine is imperfectly
vinous, the brandy being almost always sensible to delicate pa-
lates, and its effects on the stomach are proportionally injurious.
The test is therefore of real use in ascertaining the correct fabri-
cation of those wines to which brandy is added, and it will in-
variably be found that the worst wines of the growth of Portugal
and Spain are those which are the most sensible to it, or in other
words, those which contain the greatest quantity of uncombined
alcohol.   But to return to the consequences which arise in the
liquor itself from the admixture of alcohol.   It decomposes the
wine.   However slow the effects of this decomposition may ap-
pear, they are not the less certain.   The first and most con-
spicuous effect, is the loss of that undefinable lively or brisk fla-
vour which all those who possess accuracy of taste can discover
in French wines, or in natural wines; and a flatness, which must
be sensible, by the principle of contrast, to the dullest palate
which shall compare the taste of claret with that of port, or that
of hock or grave with lisbon or bucellos.   It tends equally, al-
though in a greater length of time, to destroy the union of the
colouring principle, which is well known to be deposited in port
wines, and apparently, in a great measure, from the action of
this foreign substance.   It may not be useless at the same time
to consider the influence which it must have on the quality of the
wine as a beverage.   The habitual use of wine containing, as
many of the wines of Portugal so often do, a large portion of
brandy, must be manifestly equivalent to the habitual use of
spirits, or rather, to the use of spirits and wine together.   To
this cause we may doubtless attribute the great difference in the
effects produced by an immoderate indulgence in port and sherry,
or by a similar use of claret and other French wines.   Even the
immediate effects are sensibly different, as I have said before,
and the transitory nature of the one with the permanence of the
other are too well known to be insisted on.   But the ultimate
consequences appear to be of a more serious nature.   It is well
known to physicians that diseases of the liver are the most com-
mon and the most formidable of those produced by the use of
spirits.   It is equally certain that no such disorders follow the
intemperate use of pure wine,-however long indulged in ; and
to this cause, the concealed and unwitting consumption of spirit,
as it is contained in the wines commonly drunk in this country,
                                                                    is

is doubtless to be attributed the excessive and increasing preva-
lence of hepatic affections, diseases comparatively little known
to our continental neighbours. It is sufficient to have touched
on this most important subject, on which the proposed limits of
the present essay will not allow me to dwell. It is more to my
present purpose to show, that the use of brandy in the manu-
facture of wine is founded on a mistaken principle. Having
shown that it is injurious to wine in general, by destroying its
liveliness and hastening its decomposition, I might strengthen
this assertion by mentioning that it is not used in any of the
wines of France or Germany, and that the finer wines, claret,
burgundy, and hock, are totally destroyed by it. But it is also
proper to point out its insufficiency for producing the effects ex-
pected from it, the preservation of the wine, and the retardation
of the acetous process. The former parts of this essay having
fully explained the nature both of the vinous and of the acetous
fermentation, I need not here again describe them, except to re-
mind the reader that the acetous process cannot take place
while there exists between the leaven and the sugar a dispropor-
tion in favour of the latter, and that the fermentation cannot be
re-excited if the leaven has been entirely separated by the usual
processes of racking, fining, and sulphuring, should even the
sugar have disappeared. Such wine can therefore have no ten-
dency to vinegar, and the addition of brandy if intended to pre-
vent that effect is at least superfluous. It is now to be inquired
whether brandy has any power to prevent the acetous process
from taking place, supposing that the circumstances favourable
to it are present. If brandy in small quantity be introduced into
vinegar during the acetous stage of fermentation, the process
goes on as before, and the alcohol is acetified, the produce be-
coming a stronger vinegar. This has indeed been lately denied
by Mr. Cadet, in whose hands the addition of alcohol in small
quantities appears to have had no effect on the acetous process.
From his experiments it would also appear that the addition of
alcohol in a quantity exceeding 1-17th of the fluid suspends the
acetification. In the state of ignorance in which we are re-
specting the chemical nature of that process, it does not appear
easy to reconcile these contradictory experiments. Admitting
that the experiments of Mr. Cadet are unexceptionable, it remains
certain that wine can be, as it daily is, brought into the acetous
fermentation by proper treatment or under   cir-
cumstances, although containing a far greater proportion of al-
cohol than that which appeared to him sufficient to suspend the
process. It is certainly possible that the state of combina
above described, in which the alcohol exists in the wine, m
when contrasted with the   mixture which may p   ly

T 2

place in his experiment, account for this difference of effect; or it may even happen that the action of alcohol on a process already commenced may be sufficient to account for the difference, the same alcohol applied before the commencement of the process being susceptible of the incipient changes, and being thus ultimately capable of entering into the final ones in common with the rest of the fluid. But this subject is yet obscure. A correct and varied repetition of these experiments would be necessary to render this subject thoroughly intelligible, and they may be added to the general list of *desiderata* in this department of chemistry which have already been enumerated.

" I must therefore proceed in the examination of this subject on the basis of former experience, omitting any exceptions to be drawn from these experiments, as being for the present incapable of application ; the more so that they do not appear strictly applicable to the case under review, the prevention of the change from wine to vinegar. If brandy and milk are mixed together, the acetous process establishes itself speedily, and the produce is vinegar. We have here then ample proof that brandy, in these cases, so far from checking the acetous process, increases it, and therefore, that its use, as a preservative to wine, is founded in error. I have dwelt the more on this subject because this view is opposed to all popular opinions and practices, opinions most assuredly founded on erroneous and vague analogies, drawn from some supposed preservative power residing in spirits. I am the more particular in calling to this subject the attention of those who may engage in the manufacture of domestic wines, because a notion is prevalent, that these wines are above all others deficient in durability, and cannot exist without this admixture. The effect, on the contrary, is to destroy the briskness of these wines, often the only meritorious quality they possess, while it increases their expense and diminishes their salubrity. If taste or prejudice require that wine should be stronger than it can be made naturally, or if for temporary purposes it is desirable to mix brandy with wine, it may be done, but under certain restrictions which I shall presently point out, when I have occasion to speak of the diseases incident to wine and their remedies."

The Philosophical Transactions, Part I. for 1816, has just appeared, and the following are its contents : · ·

I. On the Fire-damp of Coal-mines, and on Methods of lighting the Mines so as to prevent its Explosion. By Sir H. Davy, LL.D. F.R.S. V.P.R.I.—II. Account of an Invention for giving Light in explosive Mixtures of Fire-damp in Coal-mines, by consuming the Fire-damp. By Sir Humphry Davy, LL.D. F.R.S.

F.R.S. V.P.R.I.—III. On the Development of exponential Functions; together with several new Theorems relating to finite Differences. By John Frederick W. Herschel, Esq. F.R.S.—IV. On new Properties of Heat, as exhibited in its Propagation along Plates of Glass. By David Brewster, LL.D. F.R.S. Lond. and Edin. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. G.C.B. P.R.S.—V. Further Experiments on the Combustion of explosive Mixtures confined by Wire-gauze, with some Observations on Flame. By Sir H. Davy, LL.D. F.R.S. V.P.R.I.—VI. Some Observations and Experiments made on the Torpedo of the Cape of Good Hope in the Year 1812. By John T. Todd, late Surgeon of His Majesty's Ship Lion. Communicated by Sir Everard Home, Bart. V P.R.S.—VII. Direct and expeditious Methods of calculating the excentric from the mean Anomaly of a Planet. By the Rev. Abram Robertson, D.D. F.R.S. Savilian Professor of Astronomy in the University of Oxford, and Radcliffian Observer. Communicated by the Right Hon. Sir Joseph Banks, Bart. G.C.B. P.R.S.—VIII. Demonstrations of the late Dr. Maskelyne's Formulæ for finding the Longitude and Latitude of a celestial Object from its Right Ascension and Declination; and for finding its Right Ascension and Declination from its Longitude and Latitude, the Obliquity of the Ecliptic being given in both Cases. By the Rev. Abram Robertson, D.D. F.R.S. Savilian Professor of Astronomy in the University of Oxford, and Radcliffian Observer. Communicated by the Right Hon. Sir Joseph Banks, Bart. G.C.B. P.R.S.—IX. Some Account of the Feet of those Animals whose progressive Motion can be carried on in opposition to Gravity. By Sir Everard Home, Bart. V.P R.S.—X. On the Communication of the Structure of doubly refracting Crystals to Glass, Muriate of Soda, Fluor Spar, and other Substances, by mechanical Compression and Dilatation. By David Brewster, LL.D. F.R.S. Lond. and Edin. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. G.C.B. P.R S.

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Mr. William Phillips will publish in the course of this month, a new edition of his Outlines of Mineralogy and Geology, revised and improved. This elementary book is designed chiefly for the use of young persons. To this edition will be added, some account of the geology of England and Wales, together with a coloured map and sections of the strata; which is intended also to be published separately for the purchasers of the first edition.

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The Third Volume of the Transactions of the Geological Society will be published in the course of this month. It will be illustrated by a number of highly-finished plates, chiefly coloured.

XIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 27.—Sir E. Home furnished a supplement to his former paper on the structure of the feet of animals which have progressive motion contrary to the action of gravity. Mr. Bauer having made very accurate drawings from highly-magnified views of the feet of such animals, Sir E. has thus been enabled to correct his former observations, and even to extend them to insects. It appears that many of these animals have from one to three suckers on each foot, which, making a vacuum, enable the animal to proceed securely along a cieling with its back towards the earth. Some species of insects, particularly grasshoppers, have their feet supplied with another apparatus, that is, round elastic balls, which yield on pressure, and serve to break the violence of their fall from long leaps. On examining the structure of the flea's feet no balls were discovered, as this insect's body is so light as not to require them. Sir E. thinks that this structure of the feet of insects must furnish a new and important basis of the classification of insects; and he anticipates great advantage to science from the researches and discoveries of Dr. Leach, of the British Museum, in this department of natural history.

July 4,—J. Barrow, Esq. communicated a paper on the means of arresting or destroying the contagion of the plague, by Dr. Bernardo Antonio Gomez. The Portuguese government being anxious to prevent the plague from entering Portugal, encouraged Dr. Gomez to make some experiments, chiefly with the view of ascertaining whether the common methods of fumigating letters, or immersing them in vinegar, if received from countries where the plague was supposed to exist, were sufficient to destroy any contagious matter which might adhere to them. Dr. G. proceeded to examine the effects of fumigating a sealed letter with chlorine, having first made two or three longitudinal cuts in it; and the result proved that such fumigation must be perfectly sufficient, as every part of the letter retained the odour of the gas, which was even stronger a day or two after than at the time. He next made some experiments with vinegar, which, as well as the chlorine, changed the colour of the ink. He related the result of more than 22 experiments made with sulphuric, muriatic, and nitric acids, with burning sulphur and nitre together, &c. In order to ascertain the effects of these different acids, he caused letters to be impregnated with the odour of putrid flesh, which he found that they satisfied completely; but the chlorine he considered as the best and most efficacious of these applications, even should the letters not be cut or perforated. The fumigating

migating process of Guyton Morveau he found the most convenient. But in cases of letters coming from parts where the plague actually exists, he considers it proper either to make cuts or punctures in the letters.

Two mathematical papers, of a nature not fit for general reading, were laid before the Society, which then adjourned till Thursday, Nov. 7.

ROYAL SOCIETY OF EDINBURGH.

Papers on the following subjects have been read since our last notice:

On the Analysis of Sea Water; by Dr. Murray. In this paper the result is given of an analysis of a salt formed in the large way from the brine of sea water, and which seems hitherto to have escaped observation. It is a sulphate of magnesia and soda, crystallized in very regular rhombs, but sometimes with truncated edges and angles. It contains considerably less water of crystallization than either sulphate of soda or sulphate of magnesia, is less nauseous, and differs in other properties. It has not been applied to use, but may perhaps be employed as a purgative. Dr. Murray has also furnished a paper containing a general formula for the analysis of mineral waters, the object of which is to give one method applicable to the analysis of all waters, instead of the various methods before employed.

On the Ancient Geography of Central and Eastern Asia, with illustrations derived from recent discoveries in South India; by Mr. Hugh Murray. The author conceives that the ancients knew more respecting this quarter of the world than is generally supposed. The modern discovery respecting the course of the rivers of the Punjab, and their junction before falling into the Indus, only restores Ptolomy's map of these rivers. He endeavours, and with an appearance of some success, to show that Ptolomy's statements, carefully analysed, give a pretty correct outline of central and eastern Asia; and that the prevailing systems of d'Anville, Gosselin, &c. are founded on an undue contempt of ancient authorities, and some delusive resemblances of name.

Dr. Brewster has furnished papers—on a new optical and mineralogical property of Calcareous Spar; and on the communication of double Refraction to Glass and other substances that refract singly, by mechanical compression and dilatation; and a notice respecting some new discoveries on Light. In the notice alluded to, he mentions that he has found that water exists in nitrate of potash in the state of ice.

Dr. Gordon communicated observations to prove that the appearance called the buffy coat or inflammatory crust is occasionally seen on arterial as well as on venous blood; also other observations

vations on the muscles of the living human body during surgical operations—on muscles of limbs immediately after amputation—and on the muscles of some of the lower animals. The result is, that the muscular fibre during its contraction does not exhibit any appearance of rugæ, but remains straight; and is not perceptibly enlarged in its transverse diameter.

A criticism, by Mr. Mackenzie, on the tragedy of Bertram.—A memoir, by Mr. Alison, on the Life and Writings of the late Lord Woodhouselee;—a paper, by Mr. Cadel, on the lines that divide each semi-diurnal arc into six equal parts;—a paper, by Dr. Jackson, containing an elementary demonstration of the composition of pressures;—additional remarks by Dr. Murray on a lamp for illuminating coal mines;—and a proposal, by Mr. Kennedy, to introduce a bell-shaped bulb of glass, attached to a spiral spring fastened to the top of the barometer tube, to render the instrument less liable to damage by the concussion of the mercury.

XX. *Intelligence and Miscellaneous Articles.*

MALLEABLE PLATINA.

In the Journal of Science and Art, published at Florence, the Marquis Ridolfi has given a new process for purifying platina. Having observed the fact that no person had been able to combine sulphur with platina, he conceived the idea that, by converting all the other metals found in crude platina into sulphurets, it would be easy to purify that metal. His process is very simple. He first separates from the crude platina some of the extraneous substances usually mixed with it, and washes the remainder with nitro-muriatic acid diluted with four times its weight of water. He then melts it with half its weight of pure lead, throws it into cold water, and thus obtains an alloy, which he pulverizes, mixes with an equal portion of sulphur, and throws into a white-hot Hessian crucible; covers the crucible instantly, and keeps it in an intense heat for ten minutes. When cold, a brittle metallic button, composed of platina, lead and sulphur, is found beneath the scoria. This button he fuses with a small addition of lead; the sulphur separates itself with fresh scoria, and there remains only an alloy of platina and lead. This alloy he heats to white and in that state beats it with a hot hammer on a ??? which forces out the lead in fusion. If the alloy ??? white heat when beat, it will break. The platina ??? ductile, malleable, and as tenacious as that ob??? ammoniacal muriate. He was able to make

wire

wire with it, and leaves almost as thin as gold leaf. Its specific gravity was 22·630.

In repeating the process different times he did not always find the platina in one lump at the bottom of the crucible. It was sometimes scattered in globules among the dross. In this case he treats the mass with a little diluted sulphuric acid : the globules are soon liberated from the dross, and sink to the bottom of the crucible. They are then to be collected and washed, and submitted to the same operation of the hammer as if the platina had been found in one button with the lead.

SINGULAR PHÆNOMENON.

The following is a description of a curious phænomenon which was observed by the Honourable Company's ships Fairlie and James Sibbald, on their late passage to Calcutta :—" On the 1st October, our latitude at noon was 13° 25′ S. longitude 84° E. ; we observed quantities of stuff floating on the surface of the water, which had to us the appearance of sea-weed—but we were quite astonished to find it burnt cinders, evidently volcanic. The sea was covered with it the two next days. Our latitude on the 3d October, at noon, was 10° 9′ S. longitude 84° 20′ E. ; the surface of the water was so completely covered with the volcanic matter, that I should think it very unlikely to have been drifted any considerable distance, as it is probable it would have been much more scattered. In an old chart I had on board there is a submarine volcano placed in the same longitude, and latitude about 8° 30′ S. ; and from the great distance from any land where we found this curious phænomenon, I think there can be no other way of accounting for it than the probability of a submarine volcano existing in that neighbourhood."

In the month of July 1814 a similar phænomenon was observed in the gulph of St. Lawrence. We were informed of it by a letter from an intelligent military officer, who sent us some of the sea-water, with a sample of the ashes swept from their deck. The sea was black like ink, and for two days the sun could hardly be seen ; and when seen, his light was so much obscured by the thick atmosphere as to appear like blood. We laid the letter aside, expecting that a few weeks would bring us accounts of the breaking out of some volcano in North America, or some dreadful conflagration in the forests ; but no such accounts arriving, the letter was forgotten, till recalled to memory by the above occurrence.

By advices received from the Gold Coast it appears that General Daendels, governor in chief of the settlements in that country, has been surveying the rivers

ported to his government the expediency and practicability of acquiring land, by purchase of the natives, at a very low price : and has therefore recommended that extensive purchases should be made with a view to convert the same into plantations of cotton and coffee, and that he has already made considerable progress in clearing the land of wood, &c. The general further states, that the river Ancobar is navigable as far as the centre of the Dinkiva country, the first province of the king of Ashantee ; and he adds, that it is certain this river was navigable in the time of Bosman. The general then refers to ancient Dutch maps, copied from the Portuguese, to show that formerly the Portuguese had several establishments on the Ancobar, at which were convents of monks and christian churches, above 40 leagues in the interior of the country. The general suggests, that were the British and Dutch to agree to establish forts on each side of the river, with the consent of the king of Ashantee, not only might the whole commerce of that country be attracted, but also that of the country on this side of Long Mountains. The expense of such a project to the two governments, the general thinks, would not exceed 10,000*l.* sterling, as he conceives the king of Ashantee would supply a number of workmen to carry wood, stones, and lime, of which there is an abundance in the neighbourhood.

THE NIGER.

By the following extract of a letter from India, it appears as if the Niger did not lose itself in the interior of Africa, as generally supposed :

"Surat, 26th Nov. 1815.

" In my next I hope to give you an account of the discovery of the mouth of the *Niger*, certainly of some very large river in Africa, of which the report given by the natives is, that, after sailing *six months* upon it, you come to a part of the country where *white men* are found, or resort. The mouth of this river is insignificant, as is the case with many large streams in India ; even the Burrampooter, the Ganges, and the Indus may be quoted as examples ; but my informant entered it in his boat, and ascended it about 60 miles, and found the stream increase in magnitude the further he advanced."

STEAM ENGINES IN CORNWALL.

The average work performed by thirty-three engines in the month of May was, according to Messrs. Leans' Report for that month, 20,807,040 pounds of water, lifted one foot high with each bushel of coals consumed. During the same month the work done by Woolf's engine at Wheal Vor was 49,555,244 ; and by his engine at Wheal Abraham 56,917,312 pounds, lifted one foot high with each bushel of coals.

By Messrs. Leans' Report for the month of June, the average work of twenty-eight engines was 20,884,326 pounds lifted one foot high with each bushel of coals ; and during the same month Woolf's engine at Wheal Vor lifted 43,161,819, and at Wheal Abraham 51,476,482 pounds of water one foot high with each bushel of coals consumed.

STEAM EXPLOSION.

We have again to record an instance of culpable negligence followed by a most melancholy result, in the explosion of a steam-boiler by loading its safety-valve so as to prevent the possibility of the steam escaping. The load on the lever of the safety-valve was slipped to its outer extremity, and left to itself. We need only remark that the lever should never be of such a length, or, *vice versâ*, the weight so heavy as to occasion the least danger when placed furthest from the valve. In other words, no weight should ever be used that can by any accidental change prevent the steam from lifting the valve whenever it acquires a certain measured power.

Extract of a Letter from Marietta (United States), *dated June 7,* 1816.

HORRID ACCIDENT.—We have a painful duty to perform, in recording an unparalleled scene of human misery and anguish which occurred on board the steam-boat Washington, lately built at Wheeling, (Va.) and commanded by Capt. Shreve. She started from Wheeling on Monday last, and arrived at this place on Tuesday evening following at about 7 o'clock, and safely came to anchor opposite Point Harmer, where she continued until Wednesday morning. The fires had been kindled and the boilers sufficiently hot, preparatory to her departure, when the anchor was weighed and the helm put to larboard, in order to wear her and place her in a position to start her machinery ; but having only one of her rudders shipped at the time, its influence was not sufficient to have the desired effect, and she shot over under the Virginia shore, where it was found expedient to throw over the kedge at her stern to effect it.

This being accomplished, the crew were then required to haul it again on board, and were nearly all collected on the quarter for that purpose. At this unhappy, fatal moment, the end of the cylinder towards the stern exploded, and threw the whole contents of hot water among them, and spread death and torture in every direction. The captain, mate, and several others were knocked overboard, but were saved (with the exception of one man, who is still missing,) by boats from the town, and by swimming to the Virginia shore.

The whole town was alarmed by the explosion; every physician, with a number of the citizens, went immediately to their relief. On going on board, a melancholy and really horrible scene presented itself to view—six or eight were nearly skinned from head to foot, and others slightly scalded, making, in the whole, seventeen. In stripping off their clothes the skin pealed off with them to a considerable depth: added to this melancholy sight, the ear of the pitying spectator was pierced by the screams and groans of the agonizing sufferers, rendering the scene horrible beyond description.

The cause of this melancholy catastrophe may be accounted for, by the cylinder not having vent through the safety-valve, which was firmly stopped by the weight which hung on the lever having been unfortunately slipped to its extreme, without being noticed, and the length of time occupied in wearing before her machinery could be set in motion, whereby the force of the steam would have been expended—these two causes united, confined the steam till the strength of the cylinder could no longer contain it, and it gave way with the greatest violence.

The steam-boat was warped across the river and safely moored in deep water at Point Harmar, where it is probable she will stay several weeks, till her boiler can be repaired.

As her cylinders were all on deck, the boat has received no material injury from the explosion.

By this accident 19 people were wounded; 9 of them slightly, 10 so severely that 6 are since dead, and one man is missing.

M. Guyton de Morveau, the celebrated French chemist, died at Paris in January last. He was born at Dijon in 1737, and educated to the French bar: he held the office of advocate general to the parliament of Dijon for twenty-two years. Having a turn for the sciences, and particularly chemistry, Guyton de Morveau in 1776 founded a lectureship on chemistry, mineralogy, and materia medica, and gave the course himself for thirteen years. After publishing his nomenclature he was invited to Paris, and made a member of the Institute. His most successful discovery was that of fumigating infected places.

THE LATE RAINY WEATHER.

In Yorkshire, as appears by the subjoined letter, the effects of the late rains are not considered as likely to be disastrous.

York, 27th July, 1816.

We have accounts from different parts of this kingdom, as well as from the continent, that the quantity of rain has been excessive, and the thunder-storms unusually heavy. There has not however been, with us, a greater quantity of rain than is

usual at this season—the showers, though more frequent, have been less violent than in hot summers, and not a single thunder-storm has passed over this city. The spring was unusually dry; down to the end of June the country was greatly in want of rain; and it was actually *prayed for* in churches so lately as the 30th of June. The showers which have fallen during the present month have not exceeded the supply which the country needed; nor have they yet injured the hay, except in some low grounds; but they have doubtless retarded the commencement of the hay harvest, and even prevented any hay from being got. We sub-join a statement, in inches and tenths of an inch, of the rain fallen during the three summer months for the last six years. The account for July 1816 is only taken to the 26th inst.

	May. INCHES.	June. INCHES.	July. INCHES	INCHES. Total.
1811	3·6	2·0	3·0	8·6
1812	2·0	3·7	3·3	8·9
1813	3·6	2·5	3·0	9·1
1814	0·6	1·7	1·4	3·7
1815	3·1	1·4	2·8	7·3
1816	1·4	1·3	2·9	5·6

LIST OF PATENTS FOR NEW INVENTIONS.

To Thomas Ruxton, of the city of Dublin, esq., for a lock for fastening doors, gates, drawers, desks, trunks, boxes, port-manteaus, and other things requiring fastenings.—14th May, 1816.—2 months.

To Richard Francis Hawkins, of Woolwich, in the county of Kent, gent., for a method, plan, or principle by which a tunnel or archway may be constructed or effected under the River Thames, or other rivers, for the passage of cattle, foot passengers, and other purposes.—14th May.—6 months.

To Daniel Wilson, of Usher Street, in the city of Dublin, che-mist, for certain improved apparatus to be employed in the di-stillation of animal, vegetable, and mineral substances, and in various other processes.—14th May.—6 months.

To William Simmons, of Wigan, in the county of Lancaster, writing-master and teacher of accounts, for certain improve-ments applicable to keyed instruments to which keys are or may or can be affixed.—14th May.—6 months.

To Francis Richardson, of Queen Street, Westminster, esq., for improvements on the locks and barrels of fire-arms, and also an improvement or addition to bayonet.—28th May.—6 months.

To Philip Taylor, of Bromley, in the county of Middlesex, mer-chant, for his new method of applying fluids or liquors used in the process

processes of brewing, distilling, and sugar refining.—25th Ma,.—6 months.

To Christopher Dihl, of New Bond Street, esq., for his improvement or improvements in the making mastic cement or composition, and in the mode of working and applying the san. to useful purpose, which cement or composition he denominates "Dihl's Mastic."—25th May.—15 months.

To George Dodgson, of Shadwell, in the county of Middlesex, pump and engine manufacturer, for a method of simplifying and improving the construction of extinguishing engines and forcing pumps.—27th May.—2 months.

To Isaac Hadley Reddell, of Orange Court, Leicester Square, engineer, for improvements in or on the means of lighting the interior of offices, theatres, buildings, houses, or any place where light may be required.—27th May.—6 months.

To Robert Kemp, jun. of the city of Cork, smith and brass-founder, for improvements in the making or manufacturing locks and keys.—27th May.—2 months.

To John Heathcoate, of Loughborough, in the county of Leicester, lace-manufacturer, for certain improvements upon a machine or machinery already in use for making hosiery or frame-work knitted, commonly called stocking frame.—30th May.—15 months.

To James Ransome, of Ipswich, in the county of Suffolk, ironmonger, for certain improvements on ploughs.—1st June.—6 months.

To William Shand, of Villiers Street, Strand, artificial limb-maker, for certain improvements in the construction of artificial legs and feet made of leather and wood, acting by a lever and spiral spring.—1st June.—2 months.

To John Fowlerton, of Upper Bedford Place, Russell Square, for various improvements in beacon-buoys, can-buoys, nun-buoys, mooring-buoys, and life-buoys; which improvements are applicable to other useful purposes.—11th June.—6 months.

To Edward Light, of Foley Place, professor of music, for certain improvements on the instrument known by the name of the harp-lute, which he intends to denominate the "British Lute Harp."—18th June.—6 months.

To John Burnett, of Bristol, iron-founder, for his convolving iron axletree, for the reduction of friction and animal labour, by the application of which wheels of carriages of every description are prevented from coming off whilst travelling, and carriages are drawn with less animal labour.—20th June.—2 months.

METEOROLOGICAL JOURNAL KEPT AT BOSTON,
LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1816.	'ge of the Moon	Thermo-meter.	Baro-meter.	State of the Weather and Modification of the Clouds.
	DAYS			
June 15	19	57·	30·15	Fair
16	20	52·	30·15	Fair
17	21	59·	30·10	Very fine
18	22	69·	30·	Very fine
19	23	63·	30·10	Very fine
20	24	67·	30·20	Very fine
21	25	71·	30·22	Very fine
22	26	66·	30·42	Very fine—began to rain in the evening and continued all night.
23	27	60·	29·85	Rained violently, with gale from the NW.
24	28	59·	29·95	Fair
25	new	68·	30·12	Very fine
26	1	62·	29·85	Very fine
27	2	63·	29·89	Very fine—thick fog came on in the evening from the East
28	3	63·	30·19	Very fine—Do Do
29	4	71·	30·20	Very fine
30	5			Very fine
July 1	6	59·	29 82	Rain
2	7	65·	29·85	Fine
3	8	63·	29·93	Rain
4	9	60·	29·84	Showery, with Thunder
5	10	62·	29 90	Very fine—a thick fog at night
6	11	61·	29·93	Showery—rained hard in the even^g
7	12	67·5	29·76	Cloudy—Showers in the morning
8	13	64·5	29·78	Rain—Thunder at a distance
9	full	67·5	29·75	Fine—Wind S.SE.
10	15	62·	29·78	Rain—with Thunder
11	16	62·	29·70	Rain
12	17	61·	29·81	Rain
13	18	61·	30·	Fair—a slight shower at noon
14	19	61·	29·92	Rain
15	20	63·	29·75	Showery till noon—fine afternoon, with the wind at E.

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For *July* 1816.

Days of Month	Thermometer			Height of the Barom Inches	Degrees of Dryness by Leslie's Hygometer	Weather
	8 oClock, Morning	Noon.	11 oClock, Night			
June 27	54		54	29·60	32	Cloudy
28	52	66	55	30·04	48	Fair
29	55	72	61	·05	58	Fair
30	55	71	55	29·85	50	Fair
July 1	55	66	56	·80	59	Fair
2	59	69	55	·75	60	Fair
3	56	68	54	·75	66	Fair
4	56	60	52	·65	56	Stormy
5	54	66	55	·70	65	Fair
6	55	66	54	·71	59	Fair
7	62	66	55	·60	49	Stormy
8	64	68	56	·60	49	Stormy
9	57	66	55	·52	48	Showery
10	58	67	56	·60	36	Showery
11	57	66	55	·61	38	Showery
12	52	66	59	·78	40	Showery
13	55	65	56	·90	54	Showery
14	59	66	55	·82	45	Showery
15	57	62	57	·65	0	Rain
16	56	60	55	·60	0	Rain
17	59	68	56		0	Rain
18	57	63	56		47	Fair
19	56	60	62	·45	0	Rain
20	62	74	66	·70	59	Fair
21	64	68	56	·50	50	Stormy
22	60	69	56	·74	64	Fair
23	60	66	60	.50	59	Showery
24	58	63	56	·51	40	Showery
25	60	62	57	·59	33	Showery
26	58	66	58	·90	42	Fair

N.B. The Barometer's height is taken at one o'clock,

XXI. *On Sir* H. Davy's *Safe-Lamp.* By Dr. Ure, *M.D. Professor of Chemistry, &c. &c.*

To Mr. Tilloch.

Dear Sir, — Permit me to join my voice, to the decisive testimonies already published, in favour of Sir H. Davy's safety-lamp. I was lately present at several experiments which place in a conspicuous view, its wonderful power of giving protection against the explosion of fire-damp, so often fatal to the miner. During a visit which I paid a few weeks ago to Dublin, my ingenious friend Richard Griffith esq. mining-engineer to the Dublin Society, requested my assistance in preparing and exhibiting some experimental illustrations of the lamp, for his deservedly popular course of lectures on geology. Two lamps were tried. The cage of one was formed of iron-wire, each mesh being 1-20th of an inch in width; the other cage was of copper-wire, its meshes were each about 1-30th of an inch wide. A table with raised edges, painted so as to hold water, and a large glass receiver of the capacity of a cubic foot and a half were provided : the water was about an inch and a half in depth.

Things being in this state, a glass syphon was introduced under the edge of the receiver, and the atmospherical air was sucked out till the water rose within, to a certain mark, corresponding to a known ratio of the whole volume. A gasometer filled with the compound combustible gas obtained from acetate of potash by heat, was connected by a flexible pipe, with a brass tube which was fixed tight in an aperture made through the table under the receiver, and gas was slowly introduced into it, till the water was again displaced.

Various proportions of air and gas were used in different experiments. When the lamp with its cage screwed on, was introduced into the receiver containing eleven parts of atmospherical air and one of carburetted hydrogen, the flame enlarged, but continued moderate and of a blue lambent appearance. When six parts of air and one of gas were used, the flame was more vivid, filling the whole cage ; and when the combustible gas constituted 1-5th of the whole, the flame of the wick was extinguished very soon after the lamp was introduced, but the gas continued to burn with great violence within the cage, and its wire became distinctly red hot : yet the flame did not communicate through the meshes, so as to kindle the large volume of combustible gas by which the lamp was enveloped. At the end of the experiment the wire was found to be distorted, from its free expansion being constricted by the strong parallel wires, that constitute the exterior frame, and which serve to connect the plate at top with the body of the lamp below.

latter end of last May; to which I beg leave to subjoin such practical observations as have occurred to me on the subject.

For the accuracy of the facts, the public will of course implicitly depend upon the Report of the committee. For the deductions that are here drawn from them, I alone am answerable.

The first experiments that were tried, were to ascertain the friction of three different axle-trees:

A straight axletree,

Collinge's patent axletree,

And an axletree belonging to Messrs. Bourne, mail-coach proprietors.

These experiments were tried with two-wheeled carriages, moved by weights and pulleys. The carriages were placed first upon a road of wood, afterwards upon one of iron, perfectly horizontal: the amount of the weight which put each of them in motion, was considered as the measure of the friction.

The weight of the wheels of each carriage was deducted from the gross weight, because their weight does not affect the friction of the axletree on which they turn.

The weight of each carriage, after this deduction was made, was 7 cwt. 3 qrs. 23 lbs. that is 890 pounds avoirdupois; the weight of the wheels and axletree may be taken at a medium at 250 pounds.

	On the wooden road.	On iron.
The carriage on the straight axletree was drawn by	19 lbs. ——	11 lbs.
The carriage on the patent axletree, drawn by	15 lbs. ——	11 lbs.
Ditto on Mr. Bourne's axletree, drawn by	21 lbs. ——	12 lbs.

The differences that appeared in these experiments upon the wooden and iron roads, arose from the position of the tire of the wheels, which caused the tire to sink into, or adhere more or less to the wooden road; the difference where running on the iron road was inconsiderable between the compared axletrees.

The wheels of the two former carriages were common dished wheels; the latter with Mr. Bourne's axletree had wheels with spokes inclined in opposite directions, or, as they are called, double-dished wheels*. To estimate the power necessary to overcome

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* Where wheels are so dished or splayed, as to throw the sole of the wheel, as it sets upon the ground, beyond the line let fall perpendicularly from the end of the arm of the axletree to the ground, the box of the nave will pinch the under part of the arm of the axletree near the linch-pin, and will

overcome the friction of these axletrees, when compared with the power necessary to draw the whole load upon a common road, the effort of the horses must be stated as being equal to some given weight acting with a given velocity. Each horse drawing a mail-coach at the rate of seven English miles per hour, upon an ordinary road, may be considered as exerting a power equal to one hundred pounds. Sometimes a horse exerts nearly three times this power, and sometimes, on good roads, much less; but, perhaps, one hundred pounds may be taken as an average *.

We may fairly state, that as eleven pounds were necessary to overcome the friction of the arms of the axletree, when the carriage was loaded with 890 pounds, if the arms of the axletree were loaded as in a mail-coach, with something more than four thousand pounds, it would require fifty pounds to overcome the friction of the axles; for the power of four horses drawing a mail-coach on ordinary level roads, may be estimated at four hundred pounds, one-eighth of which, viz. fifty pounds, may be considered as the resistance occasioned by the friction of the axletrees.

Creeping.

In most carriages the arms of the axletrees are bent a little downwards, so that the wheels are four or five inches further asunder above than below. Besides this, in some carriages the arms of the axletrees are bent forwards, so that the wheels are nearer together before than behind.

To determine the effect of this construction, which by workmen is called the *creep* of the wheel, the following experiment was tried:

A two-wheeled carriage, with the axletree bent downwards in the common manner, was drawn on iron by 14 lbs.; on wood by 14½. When the wheels were 4½ inches nearer together at the front than behind, it required to draw the same carriage on wood 26 lbs. and on iron 20 lbs. Hence it appears that it is of great consequence in the formation of a carriage with bent axletrees, to secure the axletrees in their proper situation, and to

will pinch the upper part of the arm of the axletree near its shoulder. Double-dished wheels are not liable to this defect. The amount of superfluous friction that arises from the circumstance here allu'ed to, may be calculated by any mechanic, in any given position of the wheel, arising from the obliquity of the road, or from its sinking into holes.

* A variety of opinions upon this subject are held by different authors, and by different practical mechanics. As to the conclusions which I mean to draw, it is of little consequence whether the force of a horse in drawing a carriage be estimated higher or lower than what I have assumed.

F 3   prevent

prevent them from being forced out of their proper direction by the effort of the springs, or the giving way of any part of the work, which fastens the axletrees to the carriage.

The apparatus with which the following experiments were tried, is fully described in the annexed Report. By the liberal supply of men from the commissariat, I was enabled to carry on the experiments with facility.

Part of the weight in the following experiments on two-wheeled carriages was sustained by the guide-pole; the quantity of this weight was determined from time to time by a steelyard. It seldom varied; but to ascertain the ratio of any inaccuracy which might arise from this circumstance, when the carriage was loaded with nine hundred and three quarters, and when *twenty* pounds were placed on the guide-pole, it required fourteen pounds to draw it. When *forty* pounds were placed upon it, it required fourteen pounds and a half to draw it.

There could not have been a difference of more than three or four pounds in the weight upon the pole in any of these three experiments, which could not affect the draft to the amount of more than two ounces.

Of the Effects of Springs on two-wheeled Carriages.

Two carriages of similar construction, except that one had no springs, and that the other had grasshopper springs, were compared.

	cwt.	qrs.	lbs.
The carriage without springs weighed ..	3	1	0
Ditto, with springs	3	2	7
To the carriage with springs, was added	4	3	21
Making in the whole	8	2	0
The carriage without springs weighed ..	3	1	0
And carried a weight of	2	1	7
Making in the whole	5	2	7

It appears that the carriage with the springs carried 2 cwt. 3 qrs. 2 lbs. more than the carriage without springs, yet on trial it rather preceded the other.

The same carriages were again compared, substituting elliptic for grasshopper springs; 2 qrs. 7 lbs. were added to the carriage without springs, to make up the additional weight of the elliptic springs, and the springs being prevented from acting by blocks, the two carriages kept together.

The springs being permitted to act, there was added to the carriage with springs 2 cwt. 1 qr. They then kept together.
                                                               Then

Then 2 qrs. being added to the carriage with springs, it preceded the other carriage, the elliptic springs being thus brought more perfectly into play.

The gross weights of each carriage, when reduced to pounds, were nearly as follows:—The carriage with springs 1008 lbs. The carriage without springs 623 lbs. From these experiments it appears, that in the first place there was but little difference between elliptic and grasshopper springs, except what arose from the difference of their weight; and in the next place it appears, that the gross difference of weight carried by the carriages, with and without springs, was nearly as 19 to 6.

But to show the exact advantage of springs in these experiments, the weight of the wheels and axletrees of both carriages must be deducted from the gross weight, because the wheels and axletrees were moved independently of the springs, only the weight incumbent on the axletree and wheels being liable to the effect of the springs. The medium weight of the wheels and axletrees of each of these carriages was nearly 250 lbs. which being deducted from the gross weights of each carriage, leaves 758 lbs. for the one, and 373 lbs. for the other; the proportion being nearly 1 to 2.

Comparison of wooden with steel Springs.

	cwt.	qrs.	lbs.
A carriage with wooden springs, carrying ..	3	3	0
Ditto without springs	2	2	7

To each of these must be added the weight of the carriage, after deducting the weight of the wheels and axletrees, viz. 145 lbs.

Making the carriage with wooden springs	562 lbs.
That without springs	432 lbs.

Nearly in the proportion of 3 to 4; and in this state the carriages kept together when moving with a velocity supposed to be about five miles an hour.

At the slow rate at which a horse draws a *loaded* cart, the carriage with wooden springs carried 3 cwt. 2 qrs. 0 lbs. being one quarter less than when moving at a quicker rate.

Hence it is apparent that the steel springs had some advantage over the wooden springs, and that when the force of a man was employed to draw a carriage with and without springs, the advantage was not only seen but felt.

Experiments upon four-wheeled Carriages, with and without Springs.

Two four wheeled carriages, as nearly similar as might be, were placed upon the wooden platform. They were constructed in such a manner, that the load on either of them might be placed within eighteen inches of the ground, or raised to the

height

height of three feet and a half, or even eight feet above the road.

The distance between the fore and hinder axletrees in these carriages was nine feet nine inches.

These carriages were so made that either of them could be shortened, so as to bring the hind and fore axletree within six feet of each other. They were also so framed as to be stiff and strong in every direction. Of these two carriages one had the springs allowed to play, the other had not.

	cwt.	qrs.	lb.
The first was loaded with	8	0	0
Which, added to the weight of the carriage, made	17	0	0
The other	6	0	0
Which, added to the weight of the carriage, made	15	0	0

Both were connected with the peirameter, and the carriage with springs, carrying the eight hundred weight, preceded the other. To show that these carriages were similar to each other, the springs of the former carriage were now prevented from acting, and the carriage was loaded with 6 cwt.

The springs of the other carriage were then allowed to play, and the carriage was loaded with 8 cwt.; when the carriage preceded in the same manner as the first had done.

The springs of both carriages were then made free, and the axles of one of them brought within *six feet of each other;* the other remaining at *nine feet nine inches asunder.* The weight of both was brought to an equality, and both were loaded at *bottom.*

	cwt.	qrs.	lb.
The carriage with the short perch was loaded with	6	0	0
The other with the long perch was loaded with ..	5	2	0

When put in motion they kept together.

The load in the short perch carriage was then placed at the top. They both kept together.

The springs of both were then prevented from acting.

	cwt.	qrs.	lb.
The short carriage had a load at top, ...	6	0	0
The long carriage, loaded at bottom, ...	5	3	0

The long carriage rather preceded.

These experiments were not performed under favourable circumstances, as the position of the hinder axletree in one of the carriages had been accidentally deranged; but I beg leave here to report the result of experiments which had been repeatedly tried with great care at my own house.

With these carriages with the wheels at equal distances, and the springs prevented from acting: one loaded at top, the other at bottom.

The

The carriage loaded *at bottom* carried 1579 lbs.

The carriage loaded *at top* carried 1505 lbs.

Both carriages were then tried with the springs in action; but no difference appeared.

From all this I conclude, as far as my experience goes, that, *cæteris paribus*, there is very little difference *in draft* between long and short, and between high and low carriages.

It may be remarked, that the advantage of springs did not appear to be so great in those four-wheeled carriages as in the experiments on two-wheeled carriages, which I have formerly mentioned. This arose from the weight in the four-wheeled carriages not being heavy enough to bend the elliptic springs, upon which they were supported, with sufficient facility*.

Comparison of Roads.

Two roads were formed, one of loose gravel, the other of broken stone.

The carriages without springs were drawn by horses attached to the peirameter, one running on the loose gravel, the other on the broken stone; the former having an additional load of seven hundred weight.

The second carriage running on broken stone, carrying four hundred weight, besides the weight of the carriage, wheels, &c. They kept nearly together.

The same carriages were made to run, one on the broken stone, the other on the well made pavement of the court.

The carriage on the broken stone carried .. .. 5 cwt.

The ditto on the pavement carried .. .. .. 17 cwt.

The latter preceded the carriage running on the broken stone.

The difference between pavement and a road of broken stones, in this experiment, however enormous it may appear, corresponds with others which I have repeatedly tried. If, however, the stones are broken sufficiently small, not larger than an inch and half diameter, they will oppose no very great obstruction to the motion, either of a horse or of a carriage.

It is therefore of great consequence, in making a road, to have the stones broken small; and this may be effected by purchasing broken stones by measure, and not by weight. For workmen may be soon convinced, by the evidence of their senses, that it is for their interest to break stones small, when they are paid for them by measure, as the additional labour which is necessary

* It was not thought expedient to try the patience of the spectators by a reiteration of experiments, which required considerable time for each repetition. Indeed, experiments of this sort require more time and a greater attention to minutiæ than are suitable to a public exhibition.

for

for the purpose is amply paid for by the increase of bulk that is thus obtained. This I have found to be the case upon the mail-coach road between Edgeworthstown and Longford. Over eight miles of this road I have been for some years supervisor, and during that time I have had no reason to complain of the size to which the workmen break the stones with which it is repaired.

In the Report of the committee, a machine which I have contrived for giving an accurate section of a road is alluded to.

It consists of a rail of deal, twenty-one feet long, three inches wide, and five inches deep. On the top of this rail a frame of wood, eighteen inches long, slides freely: one of the perpendicular sides of the rail is covered with paper.

An arm of wood, eighteen inches long, two inches wide, and an inch thick, is connected with the side of this sliding frame, by means of a common wood screw, upon which this rod turns freely at one end, as upon a centre. Near the other end of the rod a piece of fine black lead or pencil moves horizontally, in a small pencil case, in which it is urged forward by a slender spiral spring. This long rail is to be laid on any road, the section of which is required. The moveable frame, which slides upon the rail as it is moved forward by the hand, permits that end of the moveable rod that carries the pencil, to lie upon the ground; where, as the frame advances, it rises over every obstacle, and sinks into every hollow in the road, in the same manner as the wheel of a carriage rises and sinks; the black-lead at the same time marking the rise and fall of the rod upon the paper, as the frame advances.

Besides this contrivance, a similar machine has been employed, for the purpose of delineating the path which that part of a carriage which is upon springs describes; and at the same time, the path which the axletree which is not upon springs describes.

A slip of paper several yards long, and five inches broad, was attached to one of the vertical sides of the guide-rail, which is five inches deep, and is in the middle of the platform. A perpendicular rod was fastened to the axletree of the carriage, so as to rise and fall with the motion of the axletree, as the wheels went over any obstacle, or sunk into any hollow. A black-lead pencil, pressed by a slender spiral spring, was so placed at the bottom of this rod, as to mark its progress, with all its undulations, upon the paper. A similar rod was attached to the body of the carriage, which body was upon springs. This rod was furnished in like manner with a moveable pencil, which described the path and undulations of that part of the carriage which was on springs; so that, as the pencils were close to each other,

other, the curves and inequalities of these two paths were accurately delineated, and could be examined and compared at leisure.

By means of those contrivances various problems, relative to the motion of carriages and the effect of springs, may be resolved at leisure, which could not, by any other means that I am acquainted with, be subjected to accurate investigation.

Upon the whole, I beg leave to observe, that the chief thing to be attended to is, without any comparison, the goodness of the road.

That the difference in length and height of carriages, within moderate limits, does not much affect the draft of carriages.

That carriages may therefore, except where they are obliged to turn in narrow streets, be of such a length as to permit the foremost wheels to lock round, without touching the body of the carriage.

That by lowering the centre of gravity of carriages, by placing the luggage at the bottom of the carriage, the draft is not impeded, whilst great additional security is necessarily obtained.

That no very great saving of draft can be expected from the different forms of axletrees.

That every means of saving absolute weight, in the construction of a carriage, should be adopted. This caution will be attended to by every person who considers, that in going up a hill, the ascent of which is one foot in twenty, the horses that draw the carriage must exert a force equal to one-twentieth part of the weight of the carriage and of its load, which, in a common stage-coach, is often equal to two hundred weight, and so in proportion to the acclivity of any hill.

That the application of springs to carriages, either for carrying burdens or for pleasure, tends not only to the ease of the traveller, to the safety of goods that are carried, to the preservation of the roads, and to the duration of carriages themselves, but that they also materially facilitate their draft.

That the form of those springs, provided they are properly elastic, is of no great consequence. By properly elastic, I mean *adapted* to the medium weight with which it is proposed to load them: for where the springs are strong, and the carriage not sufficiently loaded, much of the advantage of springs is found to be wanting.

It is therefore much to be wished, that some means may be obtained of proportioning the pliability of the springs to the different weights with which they may be loaded at different times. Whoever has travelled alone in a mail-coach has felt what I allude to.

And

And lastly, that wooden springs may be advantageously applied to common carts.

I have employed them in four one-horse carts, that have been in daily use, for nearly four years. These carts are usually loaded with fourteen hundred weight, and are much employed in carrying stones for the repair of roads.

I have tried straight and elliptic springs, and have employed both sorts successfully.

A piece of common tough ash, five inches and a half deep in the middle, two inches deep at each end, and three inches broad, mounted on fixed shackles at one end, and with linking plates at the other, is cheap and durable.

The iron work of the shackles may last for many years. The wooden springs may be renewed at any time for about ten shillings; and I am well satisfied that these springs will soon be in common use among common carriers.

I have the honour to be, gentlemen,

Your obliged and obedient servant,

RICH. L. EDGEWORTH.

XXIII. *Report of the Committee of Natural Philosophy, appointed by the Dublin Society, on the Experiments upon Wheel-carriages, made by Mr.* EDGEWORTH, *on the 28th of May* 1816, *and succeeding Days, at the Dublin Society's House, Kildare-street.*

Dr. LITTON having kindly undertaken, at the request of this committee, to take notes for them of the Experiments on Wheel-carriages,

Your committee proceeded to form a Report thereon; but Mr. Edgeworth having favoured them with some observations on these experiments, your committee were of opinion that such comments, with a statement of the experiments, in conformity with the notes of Dr. Litton, would answer the object of the Society better then a mere Report by the committee; they therefore beg leave to lay the statement and the comments before the Society.

The apparatus employed in these experiments was as follows: A pulley seven feet in diameter, mounted upon a carriage which can be drawn forward by men or horses. It turns upon a small centre, and has been executed with such precision as to have nearly the accuracy of a balance. Mr. Edgeworth has given the name of *petrameter* to this machine. To determine the friction arising from the motion of its axis, and the bending of

the

the rope which goes round it, the whole machine was so raised as to place the wheel in a vertical situation, and one hundred weight being suspended at each end of a rope that went round this pulley, their equipoise was overcome by placing half a pound on either one side or the other.

To compare the draft of two carriages by means of this wheel, one end of a rope passing round it must be fastened to one carriage, and the other end to another; if, then, the peirameter be drawn forward, the carriage which moves the easiest will get before the other, and by adding weights to that which gets foremost, until both proceed together, the weight thus added becomes a measure of the advantage in the construction of one of these carriages over the other, or of the roads upon which they move. It must be observed, that the draft of carriages thus compared, is not to be determined by one of them preceding the other, but by the *weight which produces an equality of draft*.

Two parallel roads, or trackways of deal plank, were laid so as to be level in every direction; all the carriages used in these experiments were guided upon the roads by a guide-rail placed between the planks. The pole of each carriage was provided with brass rollers, which ran on each side of the guide-rail to prevent the friction, and uncertainty of flaunches, and guide the carriage when drawn forward.

To overcome the *vis inertiæ* of the carriages before they were brought into competition, a detent was applied to the peirameter, so as to prevent it from revolving till the carriages had advanced some yards. Pieces of wood, five-eighths of an inch in height, were nailed upon each of these trackways, to represent the mean inequalities of a road, which had been ascertained by Mr. Edgeworth, by a contrivance of his invention, to be equal to that height.

Comparison of Axletrees by means of the Peirameter.

A carriage with a straight axletree, greased with anti-attrition composition, was compared with a carriage having Collinge's patent axletree, each weighing 3 cwt. 3 qrs. 7 lbs.

	cwt. qrs.
The straight axletree was loaded with	3 : 0
Collinge's with	3 : 2

When drawn forward on the smooth plank road, by the peirameter, they kept together.

Comparison of Axletrees by means of a fixed Pulley.

A carriage of equal weight, mounted on wheels, of Messrs. Bourne, was compared with the two former; each was loaded with 6 cwt.

	On the wooden road.	On the iron road.

A carriage with straight axletree, was drawn by .. ..⎱ 19 lbs. by 11 lbs.

Ditto,　　　　　patent axletree, .. 15 lbs. — 11 lbs.

Ditto,　　Messrs. Bournes' axletree 21 lbs. — 12 lbs.

N. B. Messrs. Bournes' wheels were of the kind called double-dished.

Comparison of the Modes of Greasing.

The patent and straight axletrees being brought to an equality of draught, when drawn forward by the peirameter, and grease being substituted for anti-attrition composition on the straight axletree, the patent, though loaded with two quarters of a hundred more, preceded the straight.

Your committee cannot vouch for the accuracy of this experiment, on account of the pressure of the crowd.

On the Effect of bending the Arm of the Axle downwards, so as to produce what is termed a Creep.

A two-wheeled carriage with the axle bent downwards, was drawn on iron by 14 lbs.; on wood by 14 lbs. or $14\frac{1}{2}$ lbs. The axle of this carriage was so altered as that the horizontal gather in front was four inches and a half, and the points of the circumference, of the wheels in front, nearest to each other, were six inches above the road. The carriage was then just drawn on wood by 26 lbs.; on iron by 20 lbs.

On the Effects of Springs on Two-wheeled Carriages.

Two carriages of equal weight and similar construction, were tried by the peirameter, one having grasshopper springs, the other without springs; that with springs carrying 8 cwt. 2 qrs. preceded the one without springs, carrying 4 cwt. 2 qrs. 7 lbs.

The same carriages were again compared, substituting elliptic for grasshopper springs; 2 qrs. 7 lbs. being added to the carriage without springs; when the springs were prevented from acting, the carriages kept together; the springs being permitted to act, there were added to the carriage with springs, 2 cwt. 1 qr.—they then kept together; on 2 qrs. being added to the carriage with springs, it preceded; the springs being by this brought more perfectly into play.

Comparison of wooden with steel Springs.

	cwt.	qrs.	lbs.
A carriage with wooden springs, carrying ..	3	3	0
A carriage without springs	2	2	7

were

were of equal draft, moving at a quick rate. At a slower rate judged to be about $2\frac{1}{2}$ miles an hour; the wooden springs carried 3 cwt. 2 qrs —being 1 qr. less.

The Efficacy of Springs in aiding Animal Exertion.

A man drew, with his utmost exertion, a two-wheeled carriage with wooden springs blocked, which was loaded with 2 cwt. a given space in nine seconds;—1 cwt. 2 qrs. were added and the springs permitted to play;—with a similar exertion, he drew it over the same space in $8\frac{1}{2}$ seconds.

N. B. It is to be observed, that in all these experiments the load on the guide-rail was made as nearly the same as could be judged by lifting; but in order to ascertain how far a difference in this respect might affect the results, the following experiment was made.—In a carriage having the entire load 9 cwt. 2 qrs. 7 lbs. the weight on the guide-rail was 40 lbs.; the carriage was just put in motion by a weight passing over a pulley of 14½ lbs. when the load on the guide-rail was only 20 lbs. the carriage was put in motion by 14 lbs.

On Four-wheeled Carriages.

Two four-wheeled carriages, as nearly similar as possible, were placed on the wooden platform. They were constructed in such a manner that the load on either of them might be placed within eighteen inches of the ground, or raised to the height of three feet and a half, or even eight feet above the road. The distance between the fore and hind axletrees in these carriages, was nine feet nine inches, and they were so made, that each of them could be shortened so as to bring the fore and hind axletrees within six feet of each other. Of these two carriages, one had the springs allowed to play, the other not. The first was loaded with 8 cwt. the other with 6 cwt. Both were connected with the peirameter, and the carriage with springs carrying the 8 cwt. preceded the other.

The springs of the former carriage were prevented from acting, and the carriage loaded with 6 cwt.; the springs of the other carriage were made free, and it was loaded with 8 cwt.; when this carriage preceded. This experiment proved that the draft of these carriages was the same.

The springs of both carriages were then made free, and the axles of one of them brought within six feet of each other; those of the other carriage remaining at nine feet nine inches asunder. The weight of both was brought to an equality, and both were loaded at bottom.

The

cut qrs

The carriage with the short perch was loaded with   6 : 0
That with the long perch with   .   ..   ..   5 : 0
    When put in motion they kept together.
The load in the short perch carriage was placed at
    the top.   They both kept together.
The springs of both were then prevented from acting.
    The short carriage had a load at top of ..   ..   6 : 0
The long carriage was loaded at bottom with   ..   5 : 3
    The long carriage rather preceded.

Comparison of Roads.

Two roads were formed, one of gravel, the other of broken
stones; two carriages without springs were connected with the
peirameter, one running on gravel, the other on stones; the
former having a load of 7 cwt. the other of 4 cwt.; they kept
nearly together.

The same carriages were made to run, one on the broken
stones, the other on coarse pavement:

    The former had a load of ..   ..   ..   5 cwt.
    The latter ..   ..   17 cwt.
    The latter preceded.
In another experiment,
    The 1st had a load of   ..   ..   ..   1 cwt.
    The 2d   of   ..   ..   ..   10 cwt.
They kept together.

Experiments were also tried in order to ascertain the advan-
tage of covering the stones with straw, but little advantage
seemed to result.

Your committee cannot close this Report without returning
their sincere thanks to Dr. Litton, for the zeal and intelligence
with which he attended and reported these experiments; and
also their perfect approbation of the unwearied attention and
skill with which Mr. William Edgeworth conducted the detail

                                   R. B. BRYAN.
                                   THOMAS BROWN.
                                   F. FOX.

XXIX.   *On the Anatomy of Vegetables; intended to substitute
many important Truths in Phytology.   By Mrs. AGNES
IBBETSON.*

To Mr. Tilloch.

SIR, — HAVING now completed the *foundation* of vegetable
life, I shall hope to send you a more regular series of dissections,
corrected from many of the errors, doubt and astonishment which
                                                        (often

(often while learning) were too apt to disorder the clearness and conciseness of the picture. So frequently has the whole now been renewed; so continually have I gone over the same yearly arrangement, that I can with more perfect perspicuity, and I hope *certainty*, show the manner in which plants are formed; the mechanism they possess; the impossibility of their being *at all sensitive*; show the power which food has on plants, the variety of alterations it will produce in their appearance, and how truly each change is adapted to the situation, soil, and aspect for which it was made.

But before I commence this account, I owe to myself and to the public, to show the manner in which my whole studies have been regulated—that they may be proper judges of the credit due to me, whether I deserve the confidence I have ventured to claim, since my offering is, I understand, thought to be a system; instead of that which it really is, an exact and unaltered daily copy of what Nature truly presents each year *in the interior of a plant*, and which any person possessing a common microscope may see, provided they will follow the picture as I have done, by a *daily review* of the increase of the interior of each vegetable, from the first of its commencement to its decay.—In explaining the manner in which I have followed the study for the last sixteen years, I may without exaggeration say, that I believe no one has ever dissected or watched plants with the unwearied diligence and patience that I have; taking up a fresh plant of the same kind every three days for nearly four years following, watching the interior picture, and pursuing *each ingredient* from its first formation to its perfection, and hence to its destruction. Convinced that a plant was only to be well known by this progressive picture, I (besides all other dissections) submitted *to this*; and have gained more knowledge, and profited more, by this method, than by any other previously tried. I have been repeatedly told that my dissections are admirable, but that my system is not admissible. I have no system,. I think I may venture to say, that any one who should see the natural specimens I could show, would be convinced in a moment, because they explain themselves. How is it possible that the specimens can be true, and yet the facts they elucidate, and bring to light, FALSE? I have with the greatest care avoided forming any system, *but* that which the dissections themselves plainly establish. To prove this, I shall bring as an example " the seeds formed in the roots," and mention the different specimens, and give the drawings as they naturally appear in the tree, and as I took them; and ask my reader whether any other explanation can be given of such a series of plain facts? In September 1810, laying open the whole root of a beech, and so, I discovered a quantity

of powder forming in the extremities of the side-roots. (Plate I. fig. 1.) Continuing to open a fresh tree as the season advanced, and following up the ingredient in the interior, I saw *that powder* soon coagulate and form into balls, which, increasing by degrees, moved in a few weeks through the root in every direction, till they all centred in the different vessels of the alburnum (fig. 2), and then mounted in a slow movement, tied together by a slight thread of the line of life. Continuing my examination, I suddenly discovered that the balls had left the alburnum cylinder, and were collected around different buds at the extremity of the last year's shoot (fig. 3). In my next specimens I saw that the balls were entirely left to themselves, while all the buds were in the very act of running up into the new shoot. In the following specimen they were regularly fixed there, the shoot being just formed by the fresh flow of the sap, and completed by this process. The next appearance showed the balls collecting at the bottom of the new shoot, aggregated into the figure of a mulberry (fig. 4). I now redoubled my attention, impatient to see what would follow; when I found that a vessel had been formed (while the balls were collecting), and had dipped into each separate bud (0,0,0). The next process was the running in of the balls into this vessel (see MM), when so many were regularly dropped into each pericarp, drawn in by degrees by means of the line of life which tied all the balls together (fig. 5). As soon as the balls entered the seed-vessel, a part surrounded them and seemed to fix them in their appropriate places, and the seed-vessel closed; but on cutting it open several times within the next fortnight, no further change had taken place; but a very great alteration in the flower, which had gained its calyx and peduncles. In less than a fortnight more the flower was sufficiently advanced to commence the fructification of the seeds; and the line of life passing through each ball, a figure began to be perceived in the interior (see *Lime-seed*), which was undoubtedly *the embryo of the plant*. And the following specimen showed most plainly, that those *very balls*, formed at the extremities of the side-roots, running through the middle *root* to the alburnum, and fixed in the seed-vessel, *were really the heart of the seed*, since the progressive picture finished by the *growth of the embryo in the ball*.

Is this a system? or, Is it not rather the exact copying of the interior progress of a plant, in which neither the imagination nor even the *reason* of the *dissector* has *aught to do?* I have blindly, I think, followed my copy; my specimens exactly show the different pictures I have explained. How then can they be just, and the explanation false, since they are regularly traced to the fructification of the seed? And how can they be nourishment,
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since we follow till the embryo *grows within,* and till the seed puts on all its outward cover? Between the commencement of this picture and the completion, above twelve specimens are drawn and exhibited, showing them in each different state. How can any one forge all these, and make them *exactly agree with the plant?* And if they are acknowledged to be true, how can such plain facts be otherwise explained? I have given this example, merely to show the manner in which I study: it is as nearly that adopted in chemistry as possible; since in botany you follow with your eye the object from state to state while growing, and must therefore perfectly understand the manner of its composition; while in chemistry the matter is decomposed and recomposed again. Both means can admit of true facts only, and each that I wish to establish will be given with equal clearness and precision. The following propositions I shall first clearly explain, as they are absolutely necessary to the obtaining a thorough knowledge of the forms and nature of vegetable life; and may be called fundamental *maxims,* which ought first to be *proved true,* to open the way for more practical laws. These will show what a plant is; and indeed, in describing and drawing up the comparative anatomy of an animal and plant, in this Magazine for August 1815, I exactly fixed those marks of division which this and the next letters will more clearly illustrate.

The chief truths I have hitherto endeavoured to make known *are these;* the proofs of which I shall enforce with as much expedition as possible.

1. That there is no perspiration in plants.
2. That there is no circulation of sap.
3. That the spiral wire is the muscle of the plant.
4. That the leaves are the lungs of the plant.
5. That the different divisions of the leaves are formed of the elongations of the bark and inner bark vessels.
6. That the hairs and instruments of that kind are the means which Nature takes to form the different juices according to their various affinities. That these figures were taken for perspiration, but are in reality liquids received from the atmosphere and flowing into the plant, not a juice running from it.
7. That the root is the laboratory of all plants.
8. That the heart of the seeds is formed in the extremities of the side-roots.
9. That the flower is also formed in the middle root, and the pollen in the tap root.
10. That the corolla of a flower is formed by bubbles of water placed in rows, and owes all its beauty, and the lightness of its tint, to the refraction and reflection of the sun on the drops of water which form its pabulum.

11. That

11. That the roots and leaves of a plant will most exactly mark not only what is the soil in which they originally grew, but the situation from which they came, whether a *water plant* or a *dry plant*, a *rock* or a *valley plant*, &c.

12. That the water, and semi-water, and rock plants alone can be said to have direct air-vessels, though I have found them in parasite and early spring plants, such as the *crocus* and *hyacinth*.

13. That the leaf owes all its mechanism to the *gatherer* alone.

When I have shown the absolute truth of all these propositions in a way, I hope, that cannot well be contradicted, I have many more with which constant dissection has acquainted me, *which will altogether* (I flatter myself) exhibit *a regular system formed by Nature, and established in truth;* which may at last force conviction even on those learned botanists, who have such a holy horror of committing themselves, that they dare not trust their superior minds to hold the scale between truth and falsehood, and try the argument by a fair examination, and by the complete consistency of the opinions.

My first proposition is, "That there is no perspiration in plants." I have already said much on this subject. I shall now therefore only recapitulate the simple facts, and the manner in which I prove that there is none. I shall first show the difference between perspiration and evaporation, since to comprehend terms clearly, *elucidates a subject* beyond any other means. *Perspiration* is a matter thrown off in its liquid state, from apertures contrived for the purpose, being a matter injurious to health, and therefore necessary to be repelled : it is often seen in drops of water on the skin. But *evaporation* is merely a sign that water exists there. It flies off in vapour unseen; and whether it is or is not condensed again, depends on the presence or absence of one of its constituents, hydrogen, which, the moment it appears, has such an affinity for oxygen that they directly join and *recompose* water. Now the chief proof which Hales gives of the perspiration of plants is, that if you place a vegetable in a bell-glass, drops of water will soon run down the interior surface. This is true ; but is easily explained. Oxygen flows in quantities from every healthy plant: it is taken from the decomposed water in the vegetable, and the hydrogen is secreted. But when a plant is oppressed and sick, it parts with this gas; and this is shown by the damp clammy feel it acquires. As soon therefore as confined under a glass without free air it begins to lose its hydrogen; which immediately flying to the oxygen it had before given out and confined, they again combine and form water. But turn a strong imagination *on the plant* while under the glass, and not one drop
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of water will be seen to ooze *from the plant*, though (if any escaped) the magnifier would make the bubbles as big as *overgrown peas*. But it escapes in air; and the joint gases again compose the water, and which runs down the glass.

The philosophers of the last century, not being in the habit of examining plants with the microscope, thought all those figures discovered on the cuticle of the leaves were uncovered bubbles of water, because they appeared clear and pellucid. But all these figures are either cuticles formed by balls standing on high pedestals, or figures of a still more curious make, resembling retorts, cucurbits, and cylinders of various sorts and sizes, divided by regular valves, and formed to produce vacuums, to separate water into its component parts; in short, established for every purpose of *chemical affinity;* by which means the juices are received from the *atmosphere*, and changed to the many compounds *necessary to compose* the various juices of the plant. That balls standing on stalks or long retorts cannot be perspiration, every one will *readily own:*—but to make the matter plainer still, take two plants of the same sort, expose one to the open atmosphere; shade the other above. The first will be probably covered with dew; the other will remain *perfectly dry*, not the smallest *drop* appearing on the leaves. Where then is the perspiration ? Place this plant in a room, and it will receive dust like any other furniture; nor will that dust agglutinate or thicken : on the contrary, the smallest breath of wind will disperse and blow it all off, leaving the leaves perfectly clear :—a positive proof that no water can ooze from the leaves, or they would exactly resemble the plant when disordered with the honey-dew, and be as nasty in appearance as they then are. And how deformed Nature would be ! for one of its *greatest beauties* is the vivid green and cleanliness of its shrubs and trees. Would not the dust stick on every part, and spoil all its beauty; or roll off in large balls, smearing the leaf as it passed? It is astonishing that this objection should not occur as so very obvious.

The next proof I shall give of the impossibility of perspiration is, that every vegetable possesses an impervious skin, through which not a drop of water can pass. To understand and be certain whether plants do or do not possess a skin capable of shutting out all outward moisture, independent of the figures or hairs already mentioned, I procured with great labour near thirty different specimens of this impervious cuticle, in which however I must observe that the pores were most conspicuous. The skins were taken from the outward cuticles of leaves, stems, and petals of various plants. Placing them on a glass, with the interior side upwards, I rubbed with the greatest care, with my finger covered with very fine *cambric*, and soon found that my conjectures

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jectures were *right;* that the mark was only that of the open-
ing pabulum below, impressed by constant motion on the upper
cuticle; since after *friction* it left behind neither pore nor pat-
tern, but an impervious skin so very fine that not even the solar
microscope could display its net-work. Their skins were almost
all alike, though some were thicker than others :—some indeed
are so very transparent that the liquid appears uncovered, till you
touch it, and prove that it is contained in a vessel, which, though
so thin, is also so strong as repeatedly to bear *detonation.* Just
the same is the cuticle with which most of the sand plants are
covered, which take in so much nutriment. Just the same is
the skin which covers most plants, but which is so very transpa-
rent, that I am often obliged to place a hair with it in my sliders to
mark that there is an object there,—as it is to be seen by the naked
eye only when placed in a cross light. With such a *skin,* then,
how can the water pass in and out of the vegetable, but by
means of the hairs? And if there was no such cuticle, how would
the decomposition of water be effected? since the liquid would
leave it at one pore, as fast as it entered at the others:—whereas,
secured by this skin, it is to be seen under the cuticle bubbling
into air, just as it does when exposed to the Galvanic wire in a
glass tube, when water is decomposed. It is by the consistency
of my plan that the truth of the whole should be tried. One pro-
position proves the next—till, copied from Nature, it forms one
circle of facts, my mind was wholly incapable of suggesting, ex-
cept by being led through them by living specimens. Sure no
more reasons can be wanting to prove the mistake which philo-
sophers have made in this respect. But there are two or three
more as convincing.

The sand plants, the rock plants, take almost all their nutri-
ment from the atmosphere, their roots being *incapable* of be-
stowing any; or at most a very trifling quantity. But if they
are to lose again in perspiration the greatest part of what they
receive, how are they to be nourished? It is the want of the
impervious skin in the rock plants, which leaves them open to
imbibe all the nutriment they require, being almost without
hairs, but receiving the dew, &c. *direct as it falls.* But the
sand plants, having a *quantity of hairs* and instruments, *have
the impervious cuticle,* absorbing through these hairs an exces-
sive quantity of nourishment, which thus enters the plant. It
was these plants that were said to perspire so much, and it was
this liquid collected from the broken instruments that they called
perspiration; whereas it is all to be seen entering the plant from
every hair, and thus meandering in vessels made for the pur-
pose.

Of what use could perspiration be to plants that have little or

no heat, no exercise, that deserves the name? What is the use of perspiration to animal life? To free the blood from its redundant water; to expel from the body those particles which from repeated circulation have become acrimonious. But there being no circulation, there is no matter to become hurtful; and as fresh matter is hourly sent into the plant in every way, it cannot want cooling; since the highest heat the plant knows is during fructification, which never exceeds 13° above atmospheric heat at that time, and can therefore be in no danger from so trifling an increase of *temperature*. What then is that matter which philosophers collected, and called perspiration? It is a matter which, instead of proceeding from the plant, is wholly drawn in from the atmosphere, and taken *from* the broken hairs; and which may be regularly traced descending from the points of the hairs, and thus from valve to valve, till it enters the plant. Had simple nutriment alone been necessary, a naked pore would have been sufficient. But to form the *juices according to chemical affinity, and specific gravity,* wanted *more instruments,* far more preparation; and this is the reason of the very curious instruments constantly seen on the leaves, and the strange figures presented to our view: few of the hairs that are not *double;* few that have not many valves, to alter the juices by degrees as they meet.

I could add many other reasons against this established idea of " the perspiration of plants," did I not fear to tire my reader, and did I not also consider that I have given amply sufficient to convince all those who are *not resolved not to be convinced.* I am sorry to say, In botany there appears such a holy horror of changing established maxims, that scarcely mathematical evidence against them will suffice. This must at once put an end to every improvement in the science. And yet no one can be more against *admitting facts without strict evidence* than I am. But examination should always be bestowed.

I now turn to my second maxim, " That there is no *circulation in plants*." When reading again (and with still stricter attention) the excellent work of Sir J. E. Smith on Botany, (the best compendium I know of what Phytology is at this time) I, to my great surprise, discovered that Mr. Knight considered both bark juice and sap as the *same liquid,* and forming but *one sort.* This at once accounts for the strange mistake *between us,* concerning the sap, and its return through the bark. I shall not stop long to consider how he can esteem as the same, two liquids so diametrically opposite in every respect, and which when analysed are so essentially different: the one formed of a sort of gluten and albumen extracted from the earth, and probably proceeding in part from the underground vegetables, of which

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the earth is full; the other drawing its foundation from the atmosphere, and composed of the narcotic principles, tannin, oils, extractive matter, gums and resins, &c. &c. Thus they are as various in their effects as unlike in their appearance; the one tanning leather, the other having no effect of this kind. But this confusion has arisen from the firs, where the turpentine is apt to spread not a little into the wood, and thus mix with the sap, which, in this case, loses a little. But its overrunning its proper bounds is no reason for reckoning the two juices the same : the one is compounded in the leaves of the tree, the foundation taken from the atmosphere ; the other proceeding from the juices of the earth, and to be considered as the liquid of the ground rather than the juices of the plant.

I shall first show how the bark juice is formed. It is composed in the pabulum of the leaves, to which the juices (taken by means of the hairs from the atmosphere) constantly *contribute* ; and when the pabulum is completed, the rest of the juices flow in large vessels from the leaf-stem to the bark. Now this is what *Mr. Knight calls* "the return of the sap." But there is not a drop of sap in it, and his mistake arises from taking the muscles for sap-vessels. However, when once this is proved a mistake, all the fabric must fall with it   That the muscles which run from the wood to the leaves, and which are commonly called the spiral wire, are incapable of carrying sap, is easily shown : two of them are perfectly without aperture; and the middle one, though hollow, contains nothing but oil to humect and lubricate the spiral, that it may not contract, and thus let out (by continual agitation) *the most important part of the plant*.

The sap has but two ways of flowing up the tree : the first is up the alburnum vessels, which continue to run up the tree for a whole week or longer, twice in the year, and which is called the barking time, because, in running in its place between the bark and wood, it detaches one from the other, and by separating allows time for the sap to coagulate and form a new row of wood ; while the sap also pushing back the bark prepares a fresh row of alburnum, allowing the wood-vessels to lengthen, and by thus increasing push through the new wood. A large collection of sap is always to be discovered at the bottom of the root at this time, serving as a reservoir for the alburnum; which plainly shows "that this ascension of the sap is managed by a different law than the flow of the sap in the wood," since that has *no reservoir below*.

The next manner of the flow of the sap is up the wood-vessels. This is a constant rill, which is however much quickened in the morning or evening; but continues more or less throughout the year.

Sir J. E. Smith is perfectly right in saying that the wood can be divided into such diminutive pieces, it is impossible that they should form sap-vessels. They are not sap vessels, but *sap vacancies;* which convey the liquid up the tree with more freedom than vessels could, as the escape of the buds horizontally would be apt by moving the vessels to twist them, and thus impede the sap; whereas the vacancies formed in the wood, move to and fro with it, only tied together every inch by a spiral wire which surrounds the top of the aperture (see Plate I. fig. 8), and by contracting and dilating brings them again to their proper places, after they have been disturbed by the passing out of the bud; and as the aperture or passage for the sap is really very large, it will bear a little reducing, without injury to the flow of the liquid. The spiral also prevents their deviating too far from their proper situation. Let a piece of wood be cut quite straight and horizontal, and it will (if much magnified) easily show the spiral vessels passing from one aperture to another (see fig. 8), preventing the wood from warping too much, or obliging it to return to its proper situation.

I shall now show Mr. Knight's opinions (as far as I can understand them), and contrast them with my own. We certainly both agree as to the existing vessels, though differing in the purpose for which they are intended. It is astonishing that, not dissecting *progressively*, he should *so well have ascertained the vessels;* this I think wonderful:—but it is only by tracing them *daily, from their first existence,* that their purpose *can be really proved.*

Mr. Knight believes that the sap flows up the *spiral vessels* in the wood; that they are the *sap-vessels,* which convey all this liquid up the tree; and that, arrived at the branches, they run up to the bottom of the leaves, and convey the returning sap to the bark downwards. I (on the contrary) am convinced " that the *spirals* are the *muscles of the tree,* can convey *no sap, that* they meander in every part of the leaf to *accelerate its motion;* and that the large vessels meeting the *spirals,* at the *bottom of the leaf,* are the *inner* bark-vessels running downward, filling the bark anew which was lost by the late winter, and which is all formed in the leaves.

My first reason against Mr. Knight's opinion is taken *from the size of the spirals.* That so small a vessel should be chosen for the purpose of nourishing a *great tree,* when sixty spirals would scarcely make a small thread, seems preposterous. The next reason against it is its extreme *twisting* and *eternal agitation,* which, in so diminutive a thread, must continually impede *the progress of the sap.* Place a stem of a plant or tree in a coloured liquor; the spirals run only in the three or four last rows of
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the wood. How comes it then that the whole wood is stained with the liquor, and in the root (where the spirals are *stopped*) double the quantity of coloured wood is discovered ? If the spirals alone received the sap, the wood should be coloured only where they *run;* it would stop entirely in the root, and only half the wood would be coloured.

I have shown that the sap runs in wood vacancies instead of sap-vessels, and how this reconciles the admirable observation of Sir J. E. Smith concerning the splitting of the wood. Now again, if the spirals carry the sap to the leaves, why put in requisition the largest vessels in the tree to carry the sap on from the leaves downwards to the bark ? why such an increase of vessels to convey the same quantity of liquid?—Surely there is a contradiction here (see fig. 9). But the whole return of the sap may be refuted by the simple truth, that *a half pint cannot contain a gallon.* Mr. Knight supposes that all the quantity of sap, which in mounting occupies ten times the size of the bark, in descending again runs into the bark-vessels filled already with their own liquid, yet does not increase their size. Thus *a...a* is the bark, and *b.........b* the wood *b b* is added to *aa*, and yet remains the same size ; nor in any degree enlarges, or doubles its number of vessels. Let it descend in ever such small quantities, it must all still pass through the stem of the tree, which is infinitely too narrow in its bark and too small to admit it; besides that it has but its inner *bark vessels descending; all the others pass round* the tree ; nor could I ever discover a vessel in the bark *containing sap:* and there is certainly no confusing together two liquids differing so much as the bark juice and sap.

That famous trial which is always mentioned so triumphantly as proving the descent of the sap, much better shows the *descent of the bark juice.* I mean cutting a gash in the tree, and its increasing in the upper and not the lower orifice of the wound. Now if the whole lump formed is cut off and dissected, it is found to be " a collection of coagulated bark juice, which of course could not proceed downwards; lumps of resin congealed, sap, and half-rotten buds. The sap drawn there by the lymphatic vessels, on account of the injury done by the cut, and the buds drawn there by the increase of nourishment *". How, if it had been sap, it could be prevented running down, I cannot conceive ; but the bark juice coagulates in a moment, and the air would soon accelerate this effect. But a more thorough proof may still be given that there is no circulation of sap, by showing that the vessels which Mr. Knight has chosen for the purpose are muscles, and can therefore contain no sap ; and that the large vessels are inner bark vessels. Innumerable authors, both

* See Phil. Mag. for Aug. 1815, p. 98.

French and English, are of this opinion, and call them so. But dissecting progressively, it is impossible to mistake the ingredients. From the birth of plants to their end, from the first of their formation to the last, the spirals perform but one part, that of proving the strength of the tree, and its cause of motion.—I now therefore turn to my third proposition.

III. *That the Spiral Wire is the Muscle of the Plant.*

To prove that the spiral wire is the muscle of a plant, and acts on and for the plant exactly as the independent muscle does for the animal body,—I shall first draw up the physical resemblance, showing in how true and perfect a manner every part suits. In the first place, muscles are to the animal body, the strength of motion. Like the wood of the vegetable, the bone may lay claim to support the figure; but the muscle is the *only source of real strength* in both. Wherever strength and motion are required, there is the muscle found; and wherever strength an d motion are requisite in the plant, there the spiral wire is dis covered. As it is involuntary motion only that can be compared to the *vegetable*, it is that only which I refer to in animals. I showed in my Comparison of animal and vegetable Life*, that the latter had neither *brain* nor *nerves*, and could not therefore have any *voluntary motion*; but that, to make it amends for this apparent privation, it possessed in a very superior degree that *in voluntary* motion to be discovered in animals; that motion which is called the *vis insita* of the muscles, belongs to *no other part*; which, when the muscle of an animal is completely divided from every other ingredient, will convulse it and keep it in a continual agitation for a time, especially if the light shines on it. This has been called the *vis insita* of the muscles. The cause of this *vis insita* in animal muscles is unknown; but in the spiral wire it is certainly *light, and moisture,* that *act on it*; since the moment it is exposed to the light (especially after being detained some time in the dark), its motion is excessive, though *perfectly inert before.* Nay, I have in my slider placed an animal muscle in one, and a spiral wire in the other, and they have both on being presented to the light moved much; but the spiral infinitely more than the animal muscle—still both light and moisture seemed to affect each, though in an unequal degree. Thus the cause of motion, the means of strength, to both muscle and spiral wire, both possessing the same *vis insita*, who can deny that the spiral is the *muscle* to the plant? It appears to me to be so positive a proof as to require no other, since the *vis insita* is the very sign of the muscle. But I shall not trust to that evidence alone, but prove also that it is the source and

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cause of all strength and of all motion to the plant. View that weak and fragile thing called the corolla of a flower: how beautiful is each petal formed! but so frail, it would seem capable of being destroyed by breathing on it:—and yet it will fold and refold into different forms, when passing from a bud to a full-blown flower; will bear exposure to an evaporating atmosphere, without being hurt; will support the change of weather, rain, and even a storm; and such is its strength, that take the lovely *convolvulus* tribe, and press the petals with your finger, and you will find a resistance beyond all conception of its force: run a thread through it to prevent its folding; and if your thread is ever so strong, it will either break it, or tear the *corolla* to pieces; but it will by no means prevent your corolla from closing. Examine what causes this strength; dissect the stripes in which the force is *evidently fixed;* you will find from *four* to *seven spirals* concealed within them. The petal being *monopetalous*, there are three or four stripes to each apparent division, and it is impossible not to see that the strength resides in them only. What astonishing force to be found in the flower of some *salvias!* View its high raised banner formed to defend the stamen and pistil from injury; press it with your finger, it is to the *stripe alone* it owes the preservation of its shape, the force it possesses. This flower has strength enough to oppose a young hornet. I saw the scarlet *salvia* attacked by one; it had entered the flower, and was attempting to reach the secret nectary below; but it was very near paying with life for its temerity; for, having inserted its head and shoulders within the inward bend of the petals, they were visibly contracting, and began to close above, and in a few minutes the creature would certainly have been drowned; for the juice within increased each moment, pressed up from the secret nectary below:—it was just dead when I took it out, but soon revived enough to be dangerous. It is certain that many flowers have the power of defending their secret nectary by closing; since during my constant watching of plants I have several times found insects thus confined; the muscle thus contracting from the heat of the insect's body, which has quite caloric sufficient to influence a spiral wire, though perhaps not our thermometers[*]. The force of the spiral is not only discovered in the flower, but still more in the leaf, as the whole health of the plant depends on the constant motion of the leaves of the trees, which merits them the name of lungs to the plant, though they have few or no air-vessels within them: but the constant motion in which

[*] insects feel cold to us, because they are so infinitely colder than our own; and we feel the cold, because we press with so much heat to them: in reality it has more warmth than the vegetable even during fructification—as I shall show in my next letter.

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the spiral wire keeps them, aided by the wind, produces an eternal change in the air around each tree. But for this, the oxygen they give out would remain (owing to its weight) under the plant: but the muscles agitating its multiplied little fans, mix all the different gases together, and keep them when the wind fails in eternal agitation. All the mechanism of the leaf is managed not only by the same stripes as the petals, but by the mechanical contrivance of the leaf-stalk. It is the gatherer on each end of the peduncle that is the chief source of motion in the leaves. It is but watching this for a few hours, to see all its different motions, and to be convinced it is to the spiral it owes them, and all its strength also. The stem of the plant possesses not one-tenth part of the motion of the peduncle, nor has it a quarter of its spiral wire, its portion of muscles being exactly equivalent to the action it is obliged to exert. In those leaves that must necessarily turn, follow the motions of the sun, open and close in the morning and evening, there are always more muscles than in the plants that do not open and close. Those plants that do not move their leaves have no spiral wire, such as the *firs, lichens, sea weeds*, &c.; but the *confervas*, that move like a worm and twist in every direction, are, like the tendrils, composed almost *wholly of muscles*. Take the spirals out of the peduncle; nay, take only a few of them, and the leaf turns no more: take them out of the tendril, and it becomes perfectly inert:—but the spiral you have taken from thence, moves on the glass for hours.

Can more and stronger proofs be given? Those leaves which have most motions have most spiral wire, and have it most twisted; and if the spiral is taken from the plant, it is perfectly inert, while the part taken from it moves eternally. These proofs, added to the *vis insita* of the spiral, must, I think, be allowed to be as positive evidence as we generally are able to procure of that which is allowed to be true; and therefore sufficient to enable me with confidence to say that the spiral wire is proved to be the muscle of the plant; and that therefore Mr. Knight's idea of the return of the sap must be a mistake, since the vessels he fixed on for the purpose are incapable of carrying any liquid except a little oil—being the muscles of the plant—and that therefore there is no circulation of the sap; but that the larger vessels are bark-vessels, which can convey no sap, but are full of bark-juice:—that on a thorough examination of the bark, there are but the inner bark-vessels which run down the tree, all the rest moving round it. I hope, therefore, I shall be thought to have proved the three propositions I undertook to show.

In my next I shall show the three following with the same degree of truth. I should for the sake; but it is difficult
ficult

ficult to unite clearness with elegance : to be exactly understood,
and to prove the propositions I advance, is *all I aim at :* to fix
the exact foundation of the anatomy of botany, is my only wish:
completing this, I think I shall die in peace, and in *the hope*
that I have not lived *quite in vain* for the science I so highly
prize, and to which I have dedicated so many years.

        I am, sir,

                Your obliged servant,

Dawlish, July 2, 1816.   AGNES IBBETSON.

Explanation of the Plate.

In giving the few specimens, though for want of room I shall
not be able to give the twelve; yet I hope I may draw a series
sufficient to thoroughly explain the formation of the seed in its
regular process. 1st. The view of the radicles and the passage
of the powder into ball: the part from *a* to *b* being generally
lost in the earth when the root is taken up, if great care is not
had to prevent the accident, but they are always to be found
in the ground. Fig. 1, natural size, is made rather *too
large.* Fig. 2 is the vessels running through the root, and all
centring in the alburnum vessels, and proceeding up to the dif-
ferent buds, but not entering them. The buds are then seen to
forsake them, and to pass up into the new shoot just formed, by
stalks growing under each bud, as in fig. 7, and thus running up
to their proper places in the new shoot. This the reason why
the new wood is always grooved, even when that shape is lost
in more advanced age—the stalks growing within the groove.
Fig. 4 is the new shoot round which the buds have arranged them-
selves, and a vessel formed to convey the balls from bud to bud,
dropping so many at each pericarp ; which then closes on them,
while the collection from which they run at LL decreases
hourly till it is all exhausted. In the *arum,* when the seeds are
about to enter the seed-vessel, if a specimen is taken horizon-
tally, the seeds may be seen for the next hour to move into the
different pericarps up the stem of the flower at fig. 4, drawn
in by the line of life at *d d.* Then follows the last scene, the
seeing those very balls fructified, which had moved from the root
upwards, and the embryo growing in the ball. The ball is cer-
tainly only the heart of the seed, and reaches from *x* to *w*, at
fig. 6. I know not how these specimens can be otherwise ex-
plained. The fructification proves it the seed ; besides that I
continue to follow it up till the seed puts on its cover and drops
into the earth. And there is no reason brought against these
numerous facts, except that it was never discovered before, and
that the flower-bud is made (Mr. Knight supposes) in the bark.
That the leaf-bud is formed there I have always shown; but that

                                             the

the flower-bud is formed in the interior is so true, that no peeler
or barker but would laugh at us for thinking it doubtful: be-
sides that it has so many different changes, all of which I have
exactly traced, that it is impossible 1 should be deceived : fol-
lowing the interior of a plant every three days, and taking up a
fresh one for the purpose, I know not how I can miss the truth
in so close an investigation, and that for near four years fol-
lowing.

Fig. 8 discloses the different manner in which the sap-vessels
appear when the specimens are cut horizontally, or perpendicu-
larly. The hollow of the *vacancies* in which the sap runs at
bbb, fig. 8, and the manner the spiral is carried round at *ddd*,
are most plain, and account for the warping of the wood and its
motion back again.

Fig. 9 is a specimen of the *sumach*, showing the bark-vessels
at *kk*, and the spirals or muscles at *ss* : the one to move the
leaf, the first to convey the juice made there to the bark. Surely,
when it is recollected what excessive investigation all plants
have undergone for the last sixteen years, my discovering so
much will not be considered as a proof of their falsehood : for,
in truth, plants had never before been dissected; for what is
cutting a plant two or three times in a year, when compared with
following its interior formation day by day? And if you are to
know how a watch is made, how can it be known, but by ex-
amining and studying all its parts and taking to pieces all its
wheels? And here is a more difficult formation; for it is not only
a piece of mechanism, but one that alters its appearance though
by slow degrees. Yet I again say, no one will or can know or
understand a process, but by daily following each ingredient, and
not allowing a single week to intervene without the examination
of the interior increase of the vegetable he is studying.

[Erratum.—P. 96, in two instances *for* substitute *r.* substantiate.]

XXX. *On the Cosmogony of Moses; in Answer to* F. E——s.
By Dr. Prichard.

To Mr. Tilloch.

Sir, — My unwearied adversary F. E——s professes in his
last letter to have a presentiment that it will call forth a reply,
and, like one of the old augurs to whom he bears more than one
point of resemblance, has taken care to ensure that his pre-
diction may come to pass. In fact, he has brought a charge
against me of having violated the *jus belli*, the law which cus-
tom has imposed on such combatants as him and me. He ac-
cuses me not only of taunting him, which was very ill-natured
and

and perverse on my part, but of putting words in his mouth
which he never uttered.   I must endeavour to get out of this
scrape as well as I can, though I fear it will be at the risk, which
1 am loth to incur, of ruffling still further the serenity of my
adversary's temper.

He challenges me to produce a passage from any of his let-
ters, in which he has affirmed, either directly or by inference,
that corals are locomotive animals.   As I have no chance of
finding a thing so ridiculous asserted *totidem verbis*, I shall con-
tent myself with collecting it by inference, or rather from syno-
nymous expressions.   But first I beg to remark, that I never
supposed F. E——s to be so beside himself as seriously to main-
tain such an absurdity ; and I should not have condescended to
notice a verbal error, however extraordinary, if he had not first
introduced that captious method of carrying on the controversy
between us.

In a paper which was published in May (Philosophical Maga-
zine, xlvii. p. 348), F. E——s has asserted that zoophytes
are *moving creatures that have life**.   These words, in the rela-
tion in which they occur, must convey to the mind of every
reader precisely the same idea as *locomotive animals*.   I have
never heard that any motions have been proved to belong to
zoophytes different from the motions of mere irritability, which
are common to this class of beings and to plants, and which
therefore cannot be thought to be alluded as characterizing
any department of animated nature.   The phrase in the text,
" moving creatures that have life," being clearly intended to
distinguish animals, it would be mere quibbling, to assign it a
sense which is equally applicable to animals and vegetables.   I
cannot then be censured for not suspecting F. E——s of such a
trick or play upon words as imputing to this expression a mean-
ing that will apply to sensitive plants, and more or less to every
vegetable tribe.   If however this was the sense in which he af-
firmed zoophytes to be moving creatures that have life, I was
mistaken in saying that he called them locomotive.   I was not
aware that he was playing upon words, and therefore misappre-
hended his meaning.

But F. E——s affirms that he had precluded any misappre-
hension of his words by a restrictive clause.   He says he had
acknowledged that the motion of zoophytes " does not precisely
accord with the idea (of locomotion) which Dr. Prichard thinks

* Lamarck, to whom F. E——s appeals in his last letter, as to a high
authority, excludes them from all share even in sensibility, and terms
them apathic and the propriety of this term may be questioned, but
the opinion which to its adoption seems to be founded on very strong
facts.

the

the original text of Genesis conveys." If these words (*of loco-motion*) were to be found in the original passage referred to, I should acknowledge that the sense now imputed to it might be discovered; but as they now make their appearance for the first time, the case is somewhat different. In my preceding letter I had defined the sense which I attributed to the Hebrew words more strictly than before, and had mentioned that they convey the idea which the LXX express by the phrase ζῷα ἑρπετά. It was natural that I should suppose that F. E——s referred to this passage, and not to my first letter, in which I had adopted the less definite translation of "locomotive animals.".

I had hopes, as Mr. Horn seemed inclined to enter into the controversy, of escaping any further trouble on the question, whether the testacea are locomotive animals; but as F. E——s still perseveres in urging that point, I must inform him that he has been fighting all this time against a mere man of straw, a phantom of his own imagination. In my first paper on this subject, I observed in general terms that zöophytes and testacea, not being locomotive animals, are excluded from a place in the creation of the third day.

I was fully aware that there were some exceptions to this re-mark; but as they are known to every body, and bore no relation (as I shall presently prove) to the question in which I was inter-ested, I did not think it necessary to notice them. If F. E——s had at first candidly conceded the truth of my observations as far as they applied to zöophytes and the greater number of the testacea, I should immediately have saved him the trouble of any further discussion on this topic. But on account of the character which the controversy between us speedily assumed, I thought it quite superfluous to make any explicit answer to his remarks. Since, however, he perseveres and at length asks me a categorical question on the subject, I have only in reply to congratulate him on having made it appear, *after consulting authorities*, as he says, that SNAILS CRAWL. I willingly con-cede the point, and beg to assure F. E——s that I never enter-tained any doubt upon the subject.

But F. E. has gained no advantage over me by proving that some of the testacea are locomotive animals. In order to form an exception to the coincidences I have traced between the succession of epochas in Nature and in the Mosaic Cosmogony, it would be necessary to show that vestiges of these creeping testaceous animals are to be found in those formations which I have referred to the first period of the original creation. This, I believe, he will find a difficult undertaking. The fact is, that although many bivalvular shells are contained in the rocks which belong to this æra, there are very few univalve and those which

have been found are of the class termed pelagian or oceanic, and appear evidently from their structure never to have furnished a dwelling to creeping molluscæ. If F. E——s can mention one which properly belongs to the class designated in the 21st and 22d verses of the first chapter of Genesis, or which contained a progressive, walking or creeping animal, I shall admit the case to be an exception to the coincidences I have pointed out.

It is evident that these coincidences depend, in a great measure, on the sense of that passage in the Cosmogony which refers to the creation of aquatic animals. In the first period the vegetable tribes were produced; to which I suppose some animal species exhibiting the phænomena of animal life in a low degree to have been associated, but only such as are very remote in their nature from the orders of aquatic animals, which are designated as beginning to exist in the next period. These last are whales or fishes, and those creatures which are termed in our English translation " moving creatures that have life."

I have compared most of the passages in the Hebrew Scriptures in which the words thus translated are to be found, and I find their sense to be somewhat more definite than I at first apprehended, being misled by the authority of the version above mentioned. The exact meaning was pointed out in my letter published in the Philosophical Magazine for April, and is clearly expressed by the words used by the LXX ζῶα ἑρπετά. This I shall now prove by a sufficient number of references.

The passages in which the creatures are designated, whose nature is the subject of dispute, are the twentieth and twenty-first verses of the first chapter of Genesis.

In the twentieth verse we read

ישרצו המים שרץ נפש חיה:

which in the literal translation of Pagninus is rendered " replebunt aquæ reptile animæ viventis."

In the twenty-first verse the creatures which had been produced are termed

כל נפש החית הרמשת אשר שרצו המים:

" animam viventem repentem quam repere fecerunt aquæ."

The distinguishing terms are שרץ in the twentieth verse, and הרמשת in the twenty-first.

The word שרץ in the twentieth verse is rendered by the LXX by the Greek word ἑρπετά. The exact meaning of it is discovered in the eleventh chapter of Leviticus, verse 20th, which is rendered in the English translation, " All fowls that creep, going upon all four, shall be an abomination to you." Here also the LXX use the word ἑρπετά.

The sense is equally displayed in the twenty-ninth verse of the same chapter, which is translated, " These things shall be
                                        unclean

unclean to you among the creeping things that creep upon the earth; the weasel, the mouse, the tortoise after his kind."

In Genesis vii. ver. 21. it is rendered " creeping things" in English, and ἑρπετὸν in Greek.

The term applied to these aquatic animals in the twenty-first verse, viz. רמש, occurs in the twenty-fourth, twenty-fifth, and thirtieth verses of the same chapter, and in each of these places is rendered by the LXX ἑρπετὰ, and "CREEPING thing" in the English translation.

It is found in Psalm civ. ver. 20. " It is night wherein all the beasts of the forest do CREEP forth."

It occurs in Deuteronomy iv. ver. 18. " The likeness of any-thing that CREEPETH on the ground." In the LXX we have ἑρπετὸν ὁ ἕρπει ἐπὶ τῆς γῆς.

Both words occur in Leviticus xi. ver. 44. " Neither shall ye defile yourselves with any CREEPING thing that CREEPETH upon the earth." The noun is שרץ and the verb רמש. The LXX have τὰ ἑρπετὰ, τὰ κινουμένα ἐπὶ τῆς γῆς.

It thus appears from parallel citations, chiefly from the Pen-tateuch, that the terms employed in describing this part of the fifth day's creation mean strictly " creeping or walking animals." We advance on safe grounds when we make an author the in-terpreter of his own words.

This interpretation is further confirmed by the old versions made before the Hebrew language was altogether extinct; viz. those of the LXX and of the Targumists, if, indeed, the latter can be called versions. I have already shown that the LXX constantly use the word ἑρπετά. Both the Targums, Onkelos, and Jonathan translate רמש and שרץ by the Chaldee word רחיש which also occurs in Genesis viii. 8. and in Levit. v. ver. 2. where it corresponds with the Hebrew words rendered " creeping things."

It seems therefore unquestionable that the animals which Moses asserts to have been called into existence in the waters during the fifth period, are designated by him as whales (viz. fishes), and all the creeping things or reptiles which inhabit this element.

It is very remarkable that M. Cuvier has shown in the course of his wonderful geological researches, which entitle him to the highest rank among the naturalists who have yet lived, that the remains of oviparous quadrupeds, of reptiles properly so called, make their first appearance in the crust of the earth exactly in the same period with those of fishes. This is another instance of the remarkable coincidences which display themselves be-tween the records of Nature and those of Moses; and I am per-suaded that the more these records are scrutinized, the more

H 2 agreement

agreement will be found between them, by those persons whose minds are not too contracted to be capable of comprehensive views, or of surveying facts on a great scale*.

F. E——s has observed, that the points in discussion do not require that he should offer any opinion on the historical facts which determine me to believe that Moses was not the original author of the Cosmogony. This seems to me somewhat strange. Let us suppose the fact to be conceded, in the proofs of which F.E——s is in no wise interested, that a record of the creation existed before the age of Moses, which contained nearly the same account as that in Genesis, and let us consider what inferences necessarily follow from this concession. First, if the latter was derived from revelation, we can scarcely refuse to allow the same origin to the former, when we consider how nearly they agree. Let any person compare the Etruscan Cosmogony cited in p. 114, vol. xlvii. with that of Moses, and say if they were derived from distinct sources. But if a revealed record of these events already existed, where could be the necessity of a new revelation? The old precept, " Nec deus intersit nisi dignus vindice nodus inciderit," forbids such a supposition. Again, if such a record existed, derived and known to be derived from revelation, there is no essential difference between the case of Moses, who compiled his narration from it, and that of the Evangelists, who framed genealogies from previously extant documents. St. Matthew could no more ascertain infallibly the correctness of the documents from which he compiled, than Moses could unerringly discriminate what was authentic from what might be superadded in the revealed record transmitted by the Patriarchs. The cases are parallel, notwithstanding F. E——s's repeated attempts to prove the contrary. To suppose either or both of those writers endowed with an infallible power of discriminating the true from the false, is to attribute to them that very *sort* of inspiration against which F. E——s, from some unaccountable reason, has so particular an objection. For my own part, I can as readily admit one sort as the other; and I observe that most of the divines who have considered the nature of the historical testimony of the Scriptures adopt the hypothesis which I have just stated.

Thus far concerning the consequences which flow from the facts which F. E——s does not care to dispute. On the *probability* that the history of the creation was known before the

* I am, however, by no means disposed to maintain that there can be no exceptions. Some facts have recently come to my knowledge, which seem likely to furnish a real exception, which, if it be confirmed, I shall not fail to avow. It is however totally in a different region from that in which F. E——s has been employed.

time

time of Moses, it is scarcely worth while to comment. It is certainly as probable that it should be revealed to the patriarchs in the first ages, as to Moses who could not have any *particular* interest in it. The most important point is the *proof* of this position. In my paper published in February (vol. xlvii. p.111 et seq.) I adduced facts which appeared to me to warrant the conclusion I drew from them; and I am confident that any person who will consider them will not find them without force. I could have adduced a greater mass of evidence, but I thought it unnecessary. If however any of your readers is disposed to investigate the subject, I refer him to a work in which, since my last letter appeared, I have seen it amply discussed. I allude to the Rev. Mr. Faber's work on the Origin of Pagan Idolatry; fifth chapter on Heathen Cosmogonies.

Perhaps it is scarcely worth while to remark, that F. E——s, who has prudently declined entering into this question, has in his last letter so far forgotten himself, as to pronounce at once the whole ground of the opinion I maintain to be a vague and visionary conjecture. On this proceeding I shall make no comment.

<div align="center">I am, sir,
Your obedient servant,</div>

Bristol, August 11, 1816.   J. C. PRICHARD.

XXXI. *On Optic Vision; with a few Remarks on the Animadversions of* F. E——s *on the Author's former Communications respecting the Cosmogony of Moses.* By Mr. ANDREW HORN.

<div align="center">*To Mr. Tillo*</div>

SIR, — IN the prosecution of my inquiries into the Mosaic Cosmogony, the subject of vision, in connexion with an inquiry into the agency of light and production of colours, greatly engaged my attention. Almost every thing of antiquity that has reached us on vision, is hypothetical. Nothing truly scientific appears to have been known, relative to the manner in which it is accomplished, before Alhazen, in the twelfth century, published his Treatise on Optics. Porta and others afterwards contributed to advance the science, by discovering the use of certain parts of the organ; but the mathematical genius of Kepler made the capital discovery, that the retina is the canvass upon which the picture of the external objects is painted. Schiener shortly after demonstrated its truth, by publicly exhibiting miniature pictures of the opposite objects upon the retina of an

<div align="center">H 3   eye,</div>

eye, stripped behind of its sclerotica and choroid membrane.
The retina being considered a propagation of the substance of
the optic nerve, its claims as the true seat of vision were never
questioned till Marriotte, from his investigations into the struc-
ture of the organ, and optical experiments, rejected them in fa-
vour of the choroides. Hence the famous controversy com-
menced between him and Pecquet, who vindicated the rights
of the retina against his hypothesis. The experiments and ar-
guments were so balanced, that scientific men were for a time
divided in their opinions. The hypothesis of Marriotte, how-
ever, at length fell into disrepute, and the general opinion ever
since has been, that the retina is the proper seat of vision.

However, after a careful examination of the various parts of
the organ, and close investigation of the phænomena attending
various and repeated experiments, I have been induced to assign
the chief function in vision to the *base* of the optic nerve, and to
conclude that the optic images are formed by caustic reflection,
and exhibited in the middle of the vitreous humour; and thus
the optic impression and position of the tangible object are re-
conciled*   Among a variety of reasons and experiments, I have
adduced the well known experiments of Daniel Bernoulli and
Le Cat, but especially the famous experiment of Marriotte made
with the patches upon the wall, as confirming the theory almost
beyond the shadow of a doubt. Since the publication of my
work, I have understood that some have questioned the truth of
the theory. As a further confirmation of it, I made the follow-
ing experiments upon Marriotte's principles. I fixed three cir-
cular white papers, at the height of my eye, upon a wall of a
pale green colour, the middle one was ten inches in diame-
ter; the two others were each about two inches in diameter,
and placed at two ... from the centre of the middle paper.
On retiring with my right eye closed, and the left eye directed
to the paper on the right hand, when at the distance of nearly
four feet from the wall, the obscurity of the middle object was
first indicated by the disc becoming invisible except a small
white ring; but from the unsteadiness of the eye in its distorted
state, I found it very difficult to make the ring appear perfectly
annular; its sphericity would now be seen interrupted in the
upper part, in an instant it would change to the lower part, and
in the next instant the interruption would be on one side or the
other. When I had retired about three inches further, I lost
the object entirely; and the whole wall between the two extreme

* See a small Treatise, published by me, "The Seat of Vision deter-
mined by the Discovery of a new Function in the Organ;" or the last edition
of Dr. Hutton's Philosophical and Mathematical Dictionary, article *Vision*.

objects, which were still visible, appeared of a uniform colour. Knowing that the pupil is always, except in myopes, dilated in obscure light and when viewing dark objects, I now darkened the chamber to a considerable degree ; and instead of the *white* central paper of *ten* inches, used in the former experiment, I substituted a *black* paper *sixteen* inches in diameter. Retiring as before, I arrived at the former distance of four feet, which I had marked, and now completely lost sight of the central black object ; while the wall and the extreme papers were seen the same as in the former experiment. If any thing were wanting to confirm the truth of the theory, nothing can be required more demonstrative than this experiment ; which shows, what I have already proved in the work itself, that the *pupil* is the cause of this defect in vision, and not any *insensibility* in the *base* of the optic nerve ; an hypothesis that introduces a most unaccountable anomaly into the nervous system.

Before I lay down my pen, allow me to make a few remarks upon the animadversions that have appeared in the Philosophical Magazine, upon some *minor* parts of my former communications. I confess I am by no means partial to " the *crambe repetita*," with which your correspondent F. E——s so fondly anticipates being treated. It has generally been found, where the appetite for this dish has been indulged, it has ended in an incurable *cacoethes*. When replies and recriminations are multiplied, without any thing new being elicited, it is pretty evident that one of the parties, at least, is prompted by something else than regard for truth. Mr. F. E——s, in his reply to Dr. Prichard*, has imputed to me the opinion, that Moses in writing the Genesis was influenced by "a circuitous inspiration ;" a charge which I defy all the art of sophistry to extract from any thing I have written. I shall not fatigue your readers by bringing them over the accumulated rubbish, through which they have been so often dragged already, in order to determine in whose premises error lies ; but shall point out the particular parts in the ground-plan which have been infringed. Moses, in so many words, has declared that the universe had a *beginning*, and that this earth, with all its inhabitants, was formed within so many periods of time. The necessary inference is, that these events never could have been known, but by a revelation from the Creator himself. But heathen nations, as well as the Hebrews, were, at the time Moses wrote, in possession of certain accounts of the *origin* of the world. From this grand principle, common to the Cosmogonies, in connexion with a few less remarkable coincidences, it has been inferred, that some *account* of the *beginning* of things

had been communicated by the Creator to some person or persons *before* the age of Moses. Mr. F. E——s here calls for direct proof: " There is in Scripture," he says, " neither hint nor trace of any revelation concerning the creation anterior to Moses." Thus, by throwing the *onus probandi*, in this negative shape, upon the shoulders of those who believe a revelation to have been given anterior to the time of Moses, he imagines that he has relieved himself of some portion of the difficulties under which he labours in his attempt at proving the Genesis a fiction. But, in the present case, he demands a species of evidence which the subject by no means requires. The question is, which of these propositions is the more probable,—that different nations have *without* a revelation obtained the notion that the world had a *beginning*, or that they derived this notion from some common source, which Moses did not think necessary to mention ? Stupidity itself would blush to hesitate between the two propositions.

I now remark that the reasoning of F. E——s upon *revelation* and *inspiration* is altogether fallacious. He argues as if the terms really were synonymous and equivalent. Luke was *inspired* to write the Acts of the Apostles; and, like Moses with respect to the Pentateuch, he was an eye-witness to many of the facts which he relates; but there was *no revelation* made to him. I could perhaps conceive, in one sense or another, what a person meant, should he tell me of a *circuitous* revelation; but when he talks of " *a circuitous inspiration*," I candidly confess, I do not comprehend what he intends by the terms. The sacred history informs us that " Moses was learned in all the learning of Egypt;" and therefore it may fairly be supposed that he was conversant with the Egyptian and other heathen Cosmogonies; and he certainly was also acquainted with the account of creation current among his own people. Now whether we suppose the revelation, by which this subject became vulgar, to have been made to Noah, Enoch, or Adam, though the tradition might not have been greatly corrupted or altered among the worshippers of the true God; yet we can scarcely suppose it to have been transmitted through so many ages, to the time of Moses, entirely perfect. If Moses then, under these circumstances, had chosen of his own accord to have written a Cosmogony, he was perfectly at liberty to have made what use he pleased of the knowledge he had acquired:—he might have related the Egyptian, or any other Cosmogony, in preference to the rest; or he might have recited the Hebrew Cosmogony with its partial errors; or he might have borrowed from all, and composed " a beautiful mythos," by an ingenious arrangement of truth and fable. But when he was *inspired* by the Creator, to record the origin of the

world

world and formation of the earth, he was no longer left to his own judgement or fancy. Still, though thus restrained, it was not necessary that his previous ideas should be erased. His mind was now so controlled by *divine influence* as to relate truth *only*, so disposed as implicitly to make whatever *additions* or *abridgements* the Creator thought proper to dictate. If any person chooses to call this *a circuitous revelation*, I will not quarrel with him, because there is a sense in which I can understand it ; but if he will term it " *a circuitous inspiration*," I protest against this as *absolute nonsense*, and appeal to the king's English.

I have to apologize to Dr. Prichard, for having inadvertently exposed him to heavy censure from his opponent, who, it seems, has fortunately discovered, through the medium of Nicholson's Encyclopædia, that " the *whole order* of testacea are *not destitute* of locomotion." Now any child, that ever saw a snail crawl, or a crab walk upon the sea-shore, could have told him the same thing ; though he might not indeed have been able to say, how far certain species of the genus *Ostrea* can leap out of the water, without consulting that scientific work. I have indeed said[*], " the production of zöophytes and testacea is justly referred by Dr. Prichard to this epoch, because they are destitute of *locomotive* powers, which Moses positively assigns to *all* the productions of the fifth period. But where is it asserted by me, that " the *order* of testacea is *destitute* of locomotion?" the language which Mr. F. E——s has attributed to me[†]. He readily perceived, that to use the simple term *testacea* would, in this instance, be of no service to his tottering—fallen hypothesis ; for, if but some species of testacea are destitute of locomotion, my phraseology is correct, though like Moses I may not have been *specific* enough for so minute a philosopher.

When Mr. F. E——s can no longer obtain " *the crambe repetita* " from the twentieth verse of the Genesis of Moses, I would recommend him to a similar case in the sixteenth proposition of the third book of Euclid's Elements.

Here he will find something precisely to his taste; for though the proposition is fairly demonstrated, yet several impossible things seem to be demonstrable consequences of it. He will thus have the honour of proposing a few *paradoxes* to that obstinate class—the mathematicians. Let him however beware.— Euclid has his admirers as well as Moses. I can assure him, he will find their *crambe repetita* quite as high-seasoned, and much harder of digestion than any he has had from the cosmologists. But would any rational person attempt to bring into discredit

the Elements of Euclid, merely on account of the difficulties attending that sixteenth proposition? Here then is a striking proof, from the most certain of the sciences, of what I have before asserted, that no author can endure the test to which F. E——s wishes to subject the Genesis of Moses. The *Systema Naturæ* must be exploded for its vague classification, and the Elements for contradiction—Euclid condemned as a sciolist, and Linnæus reprobated as a pretended physiologist, at this tribunal.

Dr. Prichard will readily excuse me for not taking his advice, of exercising myself upon the hypothesis of the German professor and our erudite countryman; although I do not think it impossible to show, from the positive declarations of Scripture and the extensive signification of the terms, that God did not found the Jewish code upon the laws of the Egyptians, or any heathen nation whatever. He must perceive that I am engaged upon subjects that afford me much greater " amusement," and more satisfaction than even the total overthrow of their hypothesis could bestow [*].

                    I am, sir,
                         Your very obedient servant,
Wycombe, August 10, 1816.   ANDREW HORN.

XXXII. *Description of the Mineralogical Cabinets and Public Libraries of Copenhagen, and of the Gymnasium of Christiania in Norway.* By LEOPOLD VON BUCH [†].

THE collection of minerals belonging to the University of Copenhagen is in fact very considerable, and, as might be expected, every thing belonging to the north is found here in extraordinary beauty. Arendal's epidote, of an extraordinary size; scapolite, crystals of yellow titanium. There fossils are first seen here in perfection. I never saw such beautiful and large zircon crystals from the syenite of Friedrichsvaern as in this collection. All the pieces are excellently kept, which is seldom the case in such large collections; and it may not be amiss to inform those who before were ignorant of that circumstance, that Professor Wad, whose merit is so great, belongs to the Wernerian school.

The royal collection in Rosenburg is also one of the most remarkable and distinguished; not from the plan of the institution,

[*] In my last communication, No. 217, p. 339, instead of " the three articles," it should read " the third article."
[†] From Travels through Norway and Lapland, translated by J. Black, with Notes by Professor Jameson. 4to. 1813.

                                                            for

for it is within these few years only, or, more properly speaking, since the direction of Professor Wad, that the enlargement and arrangement of the collection have been conducted on any thing like a scientific principle; but on account of the monstrous and colossal size of the specimens. There are pieces of Kongsberger native silver, a foot in length, and from six to eight pounds in weight. A mass of silver in its natural rock, said to be worth ten thousand dollars, is in truth by no means remarkable. But the calcedony of Iceland is of a most extraordinary magnificence. The drops of calcedony hang from the top to the bottom of the piece, like inch thick pillars behind one another. Many remain in the middle, and do not reach the bottom*. The quantity of zeolite is immense. A piece of amber from Jutland, placed on a velvet cushion, is little inferior in size to that in the Berlin collection. Large pieces of geyser sinter, which the collection lately received from Iceland, were almost in the state of conchoidal opal. Besides these magnificent specimens, the collection possesses as great a treasure of choice and well-preserved northern fossils from Arendal as the cabinet of the University, since the beautiful collection of Manthey, the counsellor of state, was purchased and brought to Rosenburg. This department is so complete, that it is almost unequalled. It were to be wished that the same provision was made for those who wish to obtain by means of ocular inspection a knowledge of the composition of the mountains of the Danish dominions, which are better known. But we look in vain for specimens of rocks in any collection of Copenhagen. The appearance of the lime-stone used in Jutland or in Faxo; the stone in which the Kongsberg mine was formerly wrought; or the figure of the large rocks on the west coast of Norway at Bergen or in Nordland, we seek in vain to discover either from a series or from individual specimens; and yet it would be so easy, and at the same time so princely, to form in a royal collection something like a picture of the interior of the whole dominions.

Professor Schumacher has also made a very beautiful and complete collection of Norwegian fossils; among them he has many things which are not to be found in other cabinets. I should doubt the possibility of showing more beautiful and distinct specimens of the *leucite* of Friedrichsvarn. The crystals are as large as the leucite of Albano; we can easily recognise the double octahedral pyramid with four terminal planes; and the white colour gives it a still stronger resemblance to the Roman

* The calcedonies of Iceland and Feroe are remarkable, not only on account of their magnificence and extraordinary beauty, but also for the various curious and interesting forms they exhibit, all of which, even the stalactitic, I consider to be crystalline shoots.—J.

leucite.

leucite. They rest insulated on hornblende in the syenite, which contains zircon in such abundance. Yet in France there is a conviction that these crystals are not leucite, but analcim (Werner's cubicite). The difference between these two fossils externally is indeed very trifling, and consists principally in the greater hardness of the analcim, and its less frequent tendency to foliated fracture. But in the chemical analysis they differ more. Leucite contains twenty-four pro cent kali, and analcim, on the other hand, ten pro cent of soda. I cannot omit noticing that the appearance of this ample collection of Arendal put me always in mind of the fossils of Vesuvius. The analogy between them is great. Here and there new and wholly unknown fossils were contained in primitive stones ; and those which were known appeared in forms seldom hitherto observed. But in both places they are numerous, and heaped together to a degree which we seldom find in an existing bed ; and were we to find all that the country of Arendal produces in such uncommon perfection at a distance from their first beds, and heaped together on the declivity of a volcano like Vesuvius, we should be as embarrassed as we now are at the appearance of so many druses of nephelin, meionite, vesuvian, hornblende and felspar, in the granular limestone on the sides of Vesuvius. The first beds of these masses may therefore have been the same as a bed in micaceous slate, or gneiss like that of Arendal ; and in this case, it must be sought westwards in the sea, or in Sardinia and Corsica ; for towards the west, the primitive rocks are to be found on the Italian coasts.

The exotic articles possessed by M. Schumacher in his collection are numerous, but of no great importance.

The treasures of the Great Royal Library are well known. But the excellent collection of books of Classen, which no stranger in Copenhagen can examine without envying, is much less known than it ought to be. General Classen bequeathed not only his books to the public, but also a sufficient fund along with them, part of which was dedicated to the erection of a suitable building for the reception of the books, and the remainder to provide a revenue for the increase of the collection. He chiefly possessed historical books. But the directors of the new institution had the good sense to give up this department entirely to the Great Library, and to confine themselves solely to natural history, the arts, and travels. They wisely judged that in this way alone it was possible for them to attain any thing like perfection ; and every one who wishes for other books, may find them with more certainty in the Great Library. The consequence is, that in Classen's library we not only find the most expensive botanical works and original travels, but also a more

complete

complete collection of even the most fugitive German and foreign publications connected with these departments, than is any where else to be met with. This library possesses a yearly revenue of four thousand rix dollars; which is more than that of the Great Library, and indeed than that of most of the public libraries of Europe.

In Copenhagen therefore there is no want of assistance from books; and in this point of view it is worthy of the capital of an extensive state.

That the town is in general beautiful and well built, all travellers are unanimous in stating; and of this we become soon convinced. After every fire, which has consumed whole streets, they have been always rebuilt more beautiful and wide, and altogether on a more convenient plan, so that the town bears in many places no resemblance to what it was before 1728, 1794, and 1807. Another species of magnificence cannot fail to strike the inhabitant of a flat country, which is little noticed in the descriptions of Copenhagen. The streets are almost everywhere at the sides paved with large oblong granite flags, and many of the canals are wholly lined with flags of a monstrous size. I conjectured at first, that they had been brought from some quarry in Norway; but I was assured by M. Wad that this immense quantity was all derived from large masses on the coast of Zealand. This is a remarkable circumstance, and deserving of attention. If so many large blocks are found in Zealand, they must necessarily have made their way over the sea; for there are no granite mountains in Zealand. In whatever manner these blocks may have been driven over to Zealand, we may easily conceive that they have been brought in the same manner to Mecklenburg, Pomerania, and Brandenburg. Large granite and gneiss blocks are even seen on the smaller islands; for instance, there are many on Femoe at Laaland. These are still further proofs that all the granite of the plains in the north of Germany, however great the distance, has been torn from the northern mountains, and not from the hills of Saxony or Silesia. We are not in possession of facts sufficient to enable us to develope the wonderful revolutions of nature, by which this may have been effected; but every observation brings us nearer to the causes, and in a few years perhaps they may be discovered.

The Gymnasium in Christiania, which bears the modest appellation of school, may be mentioned with distinction as a public establishment for education. Its merits are proved by the abilities of the teachers, and the progress made by the scholars. It supplies to a certain extent the want of a university in Norway, which has been so often warmly, but, however reasonably, always
fruitlessly,

fruitlessly, demanded by the Norwegians, as a literary centre in the interior of a remote kingdom, which constitutes more than a third part of the whole monarchy. The school, which is situated in the best part of the town, is a large building, and has a serious and dignified external appearance. It contains, besides the rooms adapted for tuition, several collections, which are not very distinguished, and the library, which is not more ornamental than useful and profitable to the town. This library is open to the citizens, and contains perhaps not many rare, but a number of useful works. It owed its origin chiefly to the collection of Chancellor Deichmann, who died about twenty years ago, and who distinguished himself by his works on the modern history of Norway. This patriotic individual bequeathed his library to the town of Christiania, well judging that it would there be productive of the greatest benefit. In the same spirit several other more recent libraries have been incorporated with it, for which they are partly indebted to an Ancker; and they now continue unremittingly to procure the most important productions of the German and Danish press, so far as the school-funds, which are by no means scanty, will allow them. How few towns of the same extent, or in the same situation, can congratulate themselves on such a library! And as it is not suffered to remain idle, we can hardly doubt that it will greatly contribute to the diffusion of knowledge.

The excellent Military Academy, which directly fronts the school-house, is an object no less remarkable. It is certainly one of the best institutions in the Danish state, and has been the means of supplying the Danish army with a great number of useful and accomplished officers. It is a pleasant sight to see the hundred cadets, who generally receive an education here, either assembled together, or in the streets. Their vivacity, their blooming complexions, and their dignified behaviour, dispel at once every idea of constraint; and we soon see when we enter the building that it is a much nobler institution than similar schools for cadets generally are: yet the institution is almost wholly supported by the contributions of wealthy individuals. The academy is indebted for the house (an elegant little palace, and perhaps the most beautiful in the town;) to the liberality of the Ancker family, by whom it was formerly inhabited; their instruments and books are legacies; and only two years ago it received from the Chamberlain, Peder Ancker, the rich library and instruments which devolved to him on the death of his brother Berndt Ancker. By these means they have been enabled from a mathematical school, which was the origin of the institution, to convert it into an academy, in which the young officers, besides the mathematical sciences and drawing, are diligently instructed

instructed in history, natural philosophy, natural history, and foreign languages. During several days of the week they practise leaping, climbing, rope-dancing, swimming, and other exercises, which Professor Treschow in Copenhagen very appropriately calls the luxury of education; but a good officer will perhaps not regret the time he spent in such exercises. It is an excellent regulation, that the cadets neither lodge nor eat in the house; they are boarded with respectable people of the town, for the purpose of avoiding the monkishness of a secluded education. They wish to bring the young people as much as possible into contact with the world, and to break them at an early period of the narrow-mindedness which so circumscribed an occupation as that of a soldier has a necessary tendency to produce. The correctness of these principles has been confirmed by experience, even in the short space of a few years. So long as the state of Denmark deems it necessary to keep up a great army, and to dedicate so much of its attention to that object, it were heartily to be wished that all the Danish officers found such a school for their formation as the Military Academy in Christiania.

XXXIII. *Account of some further Electrical Experiments by M.* De Nelis, *of Mechlin.*

To Mr. Tilloch.

Sir,— I have lately made an experiment which tends to prove the simple current with an apparatus not insulated by disks.—On placing the chain which conducts the fluid to the ground, in communication with a large plate of lead placed on the table of the apparatus, and applying the hand during the detonations of the small bottle, we feel a succession of small shocks: it then occurred to me to place the other hand on the lower cushion of the apparatus, when I instantly felt the fluid running towards the disk. Here I said to myself, Chance has confirmed to me the circle of circulation, announced to me fourteen years since by M. Lugt, a Dutch philosopher, in consequence of several very ingenious experiments made by an apparatus perfectly insulated. The apparatus of Mr. Singer is very well adapted for repeating the two first experiments of Mr. Lugt, which seem to me to prove incontestably, that the floor does not furnish the fluid to the disk, but that the wood of the table, and that which supports the disk between the cushions, &c. are the invisible conductors, which refurnish the fluid excited by
friction,

friction, and make it a continual circle by the intermedium of the conductor, while the rotation of the apparatus lasts.

For the first two experiments, it is only necessary to fix a large copper wire with a well polished knob in the lower metal of the conductor, which is supported on the column of glass; and a second similar wire in that which supports the cushion on the second column of glass. These copper wires in a perfect state of insulation ought to bend on the side opposite to the handle, in order that we may remove or bring close these knobs to the distance proper for the experiments. For instance, for the first experiment, which seems to prove that the best isolated apparatus produces the same number of sparks, and at the same degree of strength, as when the cushions communicate with the floor, we bring the knobs to the distance at which we obtain the strongest sparks. If we turn the cylinder for a whole hour, neither the number nor strength of the sparks will be diminished; and, as I have observed, it will be the same when we communicate alternately with the cushion and the conductor.

In the second experiment Mr. Lugt places a bottle on an insulator: a crooked wire which communicates with the external coating of the bottle, presents as in my experiment its knob before that of the interior coating, in order to obtain a detonation at the time of each saturation. Present in the state of insulation the knob of the copper wire which communicates with the cushion, against the stalk of the exterior coating of the bottle, and that of the conductor against that of the interior. If the distance of the knobs is proper for obtaining the strongest detonations, their force and their number will neither augment nor diminish any thing in the insulation of all the apparatus, except by making a communication alternately with the ground, either by means of the cushion or the conductor: consequently Mr. Lugt concludes, the floor contributes nothing to the charge of the Leyden phial; and the attraction which acts, is owing to the disk, which tends to resume its equilibrium, and attracts towards it the fluid excited by friction. In a note Mr. Lugt compares this circulatory action to the Voltaic pile.

When I mentioned, in two of my letters to M. De la Metherie, the experiments of the Leyden philosopher (and which were published in the Dutch language in two small volumes in 1802 and 1803), I also admitted the theory of the two fluids: I endeavoured in consequence to make them bend to the two currents. Several subsequent experiments have made me abandon this theory for that of Franklin modified.

What gives me most pleasure is to find a confirmation by facts, of the theory of elective attractions, and the tendency which all

<div align="right">bodies</div>

bodies have to attract the ignited bases which they have lost either by fermentation, friction, or simple contact. I am glad therefore that I had delayed putting the finishing hand to the account of my experiments, because the theory of the Leyden phial by the action of a single fluid becomes thereby much simpler, as well as many other facts. I think I shall be able by the end of October to send a full account of my principal experiments. In the mean time I shall describe the way in which I arrange a non-insulated apparatus.

I place a wine bottle or flask on an insulator having two stalks and knobs bent inward, to charge on one hand the interior coating, and on the other to detonate on the knob of a third stalk screwed into the metallic stand of the insulator. Instead of placing there a tube with metal wires and water, place in contact a copper rod to which a chain is to be fixed to draw away the fluid on a laminated plate of lead two or three feet long. At the end of this plate fix with wax a large copper knob as high as the rod of a Leyden jar, of the same size with the first, in such a way that its knob may present itself facing that of the roll of lead. We begin by searching for the distance, in order to obtain the strongest detonations from that which communicates with the first conductor: afterwards, having attached with soft wax a gold or silver wire to the metal of the rubber, in order to fix in the same way the other end against the exterior coating of the second bottle; we leave a separation of about a line between the interior knob and that of the roll of lead. This arrangement establishes the circle, which I obtained by my two hands.

If we turn the disk or the non-insulated cylinder of a common apparatus with a supporter and a wooden table, on which the roll of lead is placed, the fluid which we shall see incessantly passing along the chain will not pass to the floor, but will proceed by affinity or elective attraction by the metallic circle, although the latter is interrupted in the first place by the space between the two copper knobs, and afterwards by the thickness of the glass of the second coated bottle. This passage charges it with sufficient strength to exhibit the spark set out by an exciter, and to give the commotion to make it be felt with force. It appears to me, that this experiment is one of the best adapted for demonstrating the elective affinities. There is at every jet a stronger spark between the two knobs; but it appears to me worthy of remark, that after a feeble charge, the attractive force can no longer overcome the resistance which the glass of this bottle opposes to the current by the gold or silver wire, and that it takes its ordinary course by the wood of the table, and that which supports the disk. From that instant not the least fluid

is seen between the knobs, until the moment of detonating the second bottle. Ought not this fact to induce us to suspect that the glass takes up a superabundance of fluid in what is called the Leyden charge?

<div align="center">I am, &c.</div>

July 10, 1815.   Dr Nelis.

XXXIV. *Instructions issued by the French Legislature for obtaining a Patent (Brevet d'Invention) in France*.*

Motives which gave rise to the System of Patents.

It has been always admitted, that it was as just as it was useful to the progress of the arts, to secure to inventers the property of their discoveries; but to accomplish this in an advantageous manner for themselves and for the public was not so easily arranged. Some wished that exclusive privileges should be granted them, the duration of which should be unlimited, while others thought that those privileges ought only to be temporary. Lastly, according to a third opinion, it was preferable to decree rewards to them, and to render their discoveries instantly open to all. The administration frequently had recourse to this plan; but as it involved the state in considerable expenses, and did not always satisfy inventers themselves, it was necessary once more to inquire if there was any possibility of conciliating all interests. This object was at length attained by the laws of the 7th of January and 25th of May 1791, which established brevets. Titles of this description now secure, on the one hand, to artists the exclusive enjoyment of their discoveries, and give on the other, at their expiration, a very important guarantee, *i. e.* that of the preservation of several inventions, which without this plan would be imperfectly known, if not hid from the public altogether.

Formalities to be observed by those who solicit Patents, and Amount of the Sums which they must pay.

The patents delivered by the present French Government bear no resemblance to the exclusive privileges which were obtained under the ancient monarchy: they are merely a certificate given to an individual, of the declaration which he makes of having invented a machine, or a process, from the employment of which

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* Now that a free intercourse subsists between the two countries, it is to be presumed that these instructions will be found highly useful to those British patentees who wish to extend their invention, and secure the emoluments resulting from it in France.

<div align="right">a new</div>

a new branch of industry is the result. Three kinds of brevets are issued; viz. *invention, perfectionary*, and *importation*.

Patents of *importation* are granted to those who procure for our industry a process or machine known in foreign countries only. The laws of the 7th of January and 25th of May not having determined in a positive manner the duration of these patents, an imperial decree of the 13th of August 1810 ordains that it shall be the same with that of patents of invention.

Improvements in the arts often form an invention as important as the primitive discovery. It was therefore proper to give an extensive property in them by a patent. But if the French laws have gone this length, they do not regard on the other hand as improvements, any ornaments or mere changes of forms and proportions. There must be an addition to the discovery.

Several discoveries cannot be included in one brevet: each must be the subject of a particular petition. In order to obtain a title of this kind, the compliance with different formalities is indispensable.

The claimant must, in the first place, deposit at the general secretariat of the prefecture of the department where he resides, a sealed packet, containing

1. His petition to the minister of manufactures and commerce, to the effect of obtaining a brevet for five, ten, or fifteen years, according as he pleases.

2. The memoir describing the means which he uses.

3. Double sets of drawings signed by himself, or a model of the object of his discovery.

4. An inventory, in duplicate signed by him, of the pieces contained in the packet.

He must besides pay a tax, more or less considerable, according to the duration of the brevet, which cannot exceed fifteen years.

Three hundred francs (13*l.* 10*s.*) are paid for a brevet for five years.

Eight hundred francs (36*l.*) for ten years.

Fifteen hundred francs (67*l.* 10*s.*) for a brevet for fifteen years, besides fifty francs (2*l.* 2*s.*) for the fees of making out the patent.

The law admits of the duration of brevets being extended: but in order to obtain this favour, which is but rarely granted, a royal decree is necessary. A new sum is then paid in the above proportions.

The claimant must pay as a deposit with his papers, one half of the tax. He is allowed six months to pay the other half: if not paid then, the patent falls to the ground. If patentees wish to make any changes in their original petition, they must

deposit

deposit the description of their new method in the secretariat of the prefecture and pay a second tax, which is twenty-four francs (20s.) for the chest of brevets, and twelve francs for the secretariat of the prefecture. The minister for manufactures and commerce then delivers a second title, which is called Certificate of additions, changes and improvements.

Article 10, title 1st, of the law of the 25th of May regulates the destination of the sums raised from the obtaining of brevets : in the first place they go to pay the expenses of the making out and publishing the brevets, afterwards to pay the expenses of printing and engraving the brevets which have expired; and if there is any surplus, it is to be employed to the advantage of the national industry.

The secretary-general of the prefecture draws up a procès-verbal on the back of the packet placed in his hands, and he delivers to the petitioner a certificate of having so received it. The whole is afterwards addressed by the prefect to the minister for manufactures and commerce.

Principles established by the Laws in the Deliverance of Brevets or Patents.

It has been seen above, that in France there is nothing else than the certificate delivered to an individual of the declaration which he has made, of having invented a machine or process giving rise to a new branch of industry. The administration does not judge, in fact, of the merit of the inventions for which patents are solicited. Whoever has complied with the formalities prescribed by the laws of the 7th of January and 25th of May 1791 may obtain them, as these laws enact expressly that they shall be granted on *a simple request, and without previous examination.* Thus they may be applied for, for a process known to every body; the legislature having determined that they are null, and even prejudicial to those who have obtained them, if the object for which they have been granted has no existence; or if it has been known or practised before the date of the brevet. In fact, if the discovery be purely imaginary, the expenses which the patent has cost are wholly lost. If the process was already known, Article 16 of the law of the 7th of January pronounces its nullity. The rights conferred by brevets are therefore conditional only, *i. e.* they secure an exclusive enjoyment only if the patentee is really an inventor. At the first glance, it may be thought strange that titles of this nature should be given without previous examination ; but on reflection it will appear that it would have been very difficult to have found a mode better adapted to the end in view. Several motives dictated this line of proceeding : on the one hand, it was proper to

save

save the administration the embarrassment of a long and difficult examination, and the responsibility of a judgement which, if it had been unfavourable, might have given rise to charges of partiality or malignity: and on the other hand, to spare to inventers the necessity of a communication, the abuses of which they might dread. In fact, *the previous examination* would have been completely to the disadvantage of artists, since they must have communicated, without any pledge of success, processes the property of which might have been snatched from them. It would have been necessary to have submitted these processes to commissaries following the same career with themselves, and whose private interests, prejudices, or spirit of rivalship, might sway their judgements. In the most favourable point of view, the previous examination would therefore have had for a result to dissipate some absurd projects and some futile inventions: but the public, if they had been allowed to appear, would soon have done justice to-them; and if the invention had been useless, the patentee would have thrown away the expense of his brevet. This motive is sufficient, we apprehend, to diminish in the minds of artists, generally not very rich, the partialities which they have for their discoveries, and prevent them from presenting petitions without any object.

It remained to provide for the case in which a patentee should make a dangerous use of his brevet, or one injurious to the health or morals of the public. The laws of the 7th of January and 25th of May have in this case provided the means of depriving him of a privilege which he might abuse, and even of punishing him if he does. They have likewise pointed out the steps to be taken to deprive him of a right which he has usurped over some thing already known.

Nullity of Brevets, and Authorities which decide upon them.

The nullity of brevets is decided, according to circumstances, by the administrative or judiciary authorities. The minister of manufactures and commerce decides upon it when the patentee has not paid the balance of his fees, and when the inventor (without assigning a good cause for his delay) has not brought his discovery into use within the space of two years. The tribunals are to judge upon the disputes which may rise between a patentee who wishes to maintain his privileges, and any individuals who pretend that his invention was known previously to the date of his patent, either by being in use, or by description in a printed work. The interested parties must therefore use all the necessary and usual means to obtain a decision. In ordering this measure to be pursued, the law considers the patent as a property of which no person can be deprived without a due ob-

servance

servance of the established forms. Articles 12 and 13 of the law of the 7th of January, and 10, 12, and 13, of Title II. of the law of the 25th of May, regulate the method of proceeding. According to these articles, the infringers of a patent must be brought before the Juge de Paix, who, after hearing parties and their witnesses, pronounces his decision ; which, if there be no appeal, is forthwith executed.

Arrangements made since the Promulgation of the Laws of the 7th of January and 25th of May 1791.

The laws of the 7th of January and 25th of May are not the only ones which have been issued upon brevets. There exists another law, dated the 20th of September 1792, which prohibits all granting of brevets for any other objects than those connected with the arts. Petitions for patents for financial and commercial operations gave rise to this prohibition. Subsequently a decree was published, which merely concerns the mode of delivering brevets. Previously they were granted by the supreme authority, but thenceforth by the minister for manufactures and commerce. The certificate of the petition which he gives is only a provisional title : but it becomes definitive by the transmission to the patentee of the article in the royal decree which applies to his invention, when the brevets delivered in the course of every four months are published. Difficulties having arisen, whether, upon receiving the certificate of the application, the infringers of a patent might be prosecuted, or if it was necessary to wait until the patent had received the publicity procured to it by His Majesty's proclamation,—a decree of the 25th of January 1807 puts an end to these doubts, by enacting " that the duration of a patent begins to reckon from the date of the certificate which establishes provisionally this privilege." The same decree has decided that the priority of invention, in case of contestation between two patentees for the same object, is acquired by him who has been the first that has deposited at the secretariat of the department the documents which ought to accompany the claim for a patent. An arrangement in article 14 of Title II. of the law of the 25th of May had prohibited the obtaining of brevets by what is termed *actions*. This was abrogated by a decree of the 25th of November 1806, on the representations of some individuals that it would prejudice the interests of inventers, inasmuch as it would deprive them of an easy method of taking advantage of their discoveries.

It sometimes happens that patentees address themselves to Government, in order to obtain recompenses as the authors of important discoveries. It is impossible to listen to all their demands in this respect. Article 11 of the law of September 12,
1791,

1791, prohibits the granting of particular encouragements to those who have provided themseves with a patent. This resolution was adopted, upon the consideration that no recompense is due to those inventers who reserve to themselves the exclusive enjoyment of their discoveries; and that those persons only merit favour, who render their discoveries of free and common use, and thus add to the welfare of society, which all governments ought to seek incessantly to ameliorate.

Paris, Oct. 30, 1813.   COUNT D. SUSSY,
Minister of Manufactures and Commerce.

XXXV. *On Fiorin Grass. By* WILLIAM RICHARDSON, *D. D.*

To Mr. Tilloch.

SIR, — I YESTERDAY received, through Mr. Farey, the 217th number of your valuable Magazine, containing a geological letter of mine, addressed to my friend The Countess of Gosford, communicated to you by Mr. Farey, who, I suspect, received it from my worthy and much lamented friend Mr. Johnes, member for Cardiganshire, lately deceased.

That letter was nearly limited to *facts*—while " *the observations to which these facts give rise, and the inferences to be drawn from them, were reserved for another letter*," as I there state.

This *second* letter to my noble friend, I now transmit to you, conceiving the first would be imperfect without it.

Mr. Farey has been pleased to add, and you to publish, "*Some preliminary remarks and illustrative notes:*"—upon these I must request you to indulge me in a few observations:

Mr. Farey, after some very flattering expressions, is pleased to lament " that my attention should have been entirely diverted from geology, my proper pursuit, in order perseveringly to attempt the conversion of English farmers,—*a vain attempt.*"

Vain enough no doubt, but not entirely desperate; for, while the great body of the *English* agriculturists are obstinately prejudiced against any thing they are not used to, and reject novelty with contempt,—yet still there are a few

> " ———— quibus arte benignâ
> Et meliore luto finxit præcordia Titan."

We must not therefore complain,

> " Si reliquos fugienda patrum vestigia ducunt."

My exertions too have not been entirely fruitless. Our own *East Indies* enjoy the luxuriance of this succulent grass. *St.*

I 4   *Helena*

Helena cultivates it successfully. *Denmark,* with avowed grati‑ tude, exults in the complete establishment of this vegetable, equally productive in their cold climate and in our own. *Russia* too sent a *Savant* to Clonfecle to learn Fiorin culture.

You must forgive me for obtruding a few words on a subject which, though not in your immediate department, was intro‑ duced in your own Magazine; and for showing that, although it may not be entitled to the name of *science,* yet it has always been treated by me *scientifically*.

You, Mr. Tilloch, who have occasionally published my geo‑ logical papers, know that my method is simple. I first establish *facts,*—then proceed to the *inferences* that follow directly from them. In agriculture I pursue the same line; for, when my ob‑ ject was to introduce the culture of *Fiorin grass,* by impressing on the world an high idea of its value, I commenced by esta‑ blishing its curious properties as my *facts;* and then showed the important consequences that must *necessarily* follow its pretty general culture.

As Mr. Farey states that I have *failed,* he brings the question to issue between the *English farmers* and me. I must therefore first show that my *facts* were well established.

Seldom indeed has such a mass of evidence been produced.— It is now eight years since I first publicly invited inspection at Clonfecle; and every year since I have been visited by numbers, and among others by many of the nobility and prelates of my country, who have unanimously in the face of the public borne testimony to the truth of my *facts,* that is chiefly, immensity of produce, and superior quality of my hay.

I have now a better opportunity than ever of convincing those who are disposed to doubt the truth of my facts. The liberality of the *Irish Farming Society* has held out as encouragement, magnificent premiums for the best Fiorin crops in 1816. I know of several candidates; and am myself one, though with little pro‑ spect of success, as the premiums are limited to *new* crops, and my good grounds are all long under Fiorin.

Here incredulity will be deprived of its usual excuses; care‑ less inspectors,—partial to me—and chosen by myself! But in this instance, the *facts,* that is, *quantity* and *quality* of produce, will be established by persons nominated by the Farming Society, and jealous competitors will take care that I do not deceive.

Secure of respectable judges, I shall avail myself of the wretched ground I am limited to, and *brave* the English farmers, who, as Mr. Farey tells us, *are not to be convinced;* and I now in *June* tell them, that in *October* I shall exhibit crops of hay, of superior quality, *treble* and even *quadruple* the quantity they
themselves

themselves are used to raise, with all their boasted agricultural skill, from their best grounds; while much of mine shall be found growing on land, from most of which that same great agricultural skill of theirs could not extract *any* produce—to wit, cold, wet, peaty ground, burned down (for greed of ashes) to a tough, viscid, incombustible moory clay;—and also *peat moss* twice cut out for turf, and now brought down to within twelve inches of the perpetually stagnant water, and often flooded.

My inspectors also will see, and I hope report (though not for competition) that the rest of my meadows retain their usual value, and that my *ninth* and *tenth* successive crops of Fiorin, growing on *light dry upland* ground, have not in the least fallen off.

I like to taunt incredulity, and therefore tell these unbelieving farmers of Mr. Farey's, that the grass they reject, thrives, and *luxuriates* equally on the top of the mountain and bottom of the valley; thanks to my noble friends the Marquises of Hertford and Abercorn for enabling me to establish this important *fact*, on their respective mountains. Nor, taking the question *a priori*, is it unreasonable to ask these sceptics to concede that this same Fiorin grass, already in possession, and almost exclusively, of every green mountain in our islands, must, when fostered by man,—stimulated by manure,—and protected from its enemies, *thrive* and *luxuriate* in the same soil and elevation where it grows spontaneously without any of these aids.

Had Mr. Farey's obstinate friends the faculty of *drawing conclusions*, they would soon find to what stupendous consequences this hardy habit of Fiorin must *necessarily* lead.

Our mountains are desolate, because they will not produce food for *man* without manure—nor winter sustenance for his *domestic cattle*, to afford him *manure* and *milk*. Give him hay in abundance (as Fiorin will rapidly and cheaply), all his difficulties vanish. Hay produces *milk* and *manure*. Hence *potatoes, rye,* and *black oats*.

Great proprietors may soon restore the population of the *mountain Highlands of Scotland*. Enable the holders of *small tacks* to supply themselves with hay, they cease to be claimants on shares of the widely extended pastures, or rivals of the wealthy graziers, and sheep feeders. A very few acres in the *glens* and *valleys* will enable them to feed themselves, and to derive their other necessaries from their domestic industry.

What a field is here opened for the establishment of a manufacturing population! Cheap land—food easily raised—fuel in abundance. Nor is weighty capital required:—the attentive eye and fostering hand of the proprietor are nearly all that is requisite,

But,

But, Mr. Tilloch, I am wandering from your proper depart ment, and from what Mr. Farey is pleased to call *my own proper pursuit.* I must return to it. And as he requests me " to give an attentive reading, and consideration, to his paper on *the Strata of Antrim,* in your xxxixth volume, and that I will, ere long, furnish *you,* at some length, with my candid and free remarks on this paper, compiled chiefly from my own writings:"—

I reply, that I shall be happy to do so, so soon as *you* or *he* can furnish me with the paper referred to, which in my remote and retired situation has not reached me.

Moy, Ireland, June 1816.   W. RICHARDSON.

XXXVI. *Experiments relating to the Impurities of Hydrogen as it is commonly obtained. By* M. DONOVAN, *Esq.*

Read to the Kirwanian Society of Dublin, May 15.

THE process employed for obtaining hydrogen is the decomposition of water by means of a metal. For this purpose zinc and iron are commonly used, with dilute muriatic or sulphuric acid[*]. But the gas thus produced is variable in some of its properties: its odour, and the colour of its flame, are not always the same: it therefore contains foreign matter.

Experiments which required very pure hydrogen compelled me to pay some attention to its common impurities, and to the best means of removing them.

A very small Woulfe's apparatus was arranged, the first bottle containing caustic ammonia, the second lime-water, and the third common water. A long continued stream of hydrogen was passed through this series, from the vessel in which it was generating; and the pressure of several inches of water was applied. By this process, the ammonia acquired a fetid smell. When mixed with muriatic acid, it exhaled the odour of sulphuretted hydrogen; and paper moistened in acetate of lead was blackened, when held over the fluid. The lime-water contained carbonate of lime; and when the clear fluid was poured off, and mixed with muriatic acid, a minute quantity of white powder was deposited, which when separated burned blue like sulphur. The smell of the hydrogen obtained was, by these means, altered: it had now an odour precisely like phosphorus. It burned with a fine green flame; whereas before it burned blue.

[*] The gun-barrel process is so very troublesome and difficult to manage, that, I believe, it is seldom resorted to. It affords, however, a very pure hydrogen.

Wishing

Wishing to try if hydrogen, procured by means of zinc, contain sulphur, I repeated the experiment with that metal. Sulphuretted hydrogen and sulphur were obtained as before; and the gas gave the same smell and flame as the former.

By repeating the process, with muriatic acid, it was proved that the sulphur had not been afforded by the sulphuric acid. And trials on different specimens of iron and zinc offered the results already stated.

Thus common hydrogen contains an admixture of sulphuretted hydrogen which is not absorbed during the passage of the gas through the water in which it is received; and this admixture affects both its smell and the colour of its flame.

From these facts, it appeared that the water resulting from the combustion of common hydrogen should contain sulphuric acid. A large quantity of hydrogen was therefore collected in new dry bladders provided with pipes and stop-cocks, and the combustion was suffered to go on slowly, in a new and perfectly clean globe, kept cool under water. The water produced was tested with muriate of barytes, which caused a precipitation; it also reddened water tinged with violets.

The experiment was repeated with this difference, that the gas, previously to its reception into the bladders, was suffered to remain over water for twenty-four hours, and was frequently agitated. In this case, the water produced did not afford a precipitate with muriate of barytes.

During the revolution which took place in chemistry towards the end of the last century, there was a controversy concerning the product of hydrogen burnt in oxygen. The advocates of the new doctrine stated that the result is water. The opposite party maintained that an acid is the product. Acid was, in some cases, certainly found; but it was conceived by the framers of the new system to be formed by the combustion of a little azote, from which it is difficult to free oxygen. In other trials no trace of acid could be detected. The foregoing statements render it probable that, when acid was produced, it might have been the sulphuric, but so much diluted as to be difficult to recognise; and when there was no trace of acid, the hydrogen had perhaps lain so long over water that the sulphur was taken up entirely.

The hydrogen, which had been freed from sulphur and carbonic acid, had a remarkable smell of phosphorus. This might be really owing to phosphorus, for some kinds of iron have been proved to contain it; or it might be owing to arsenic. Either of these would be acidified by passing the impure hydrogen through nitrous acid, and the gas might be obtained in a state of purity.

A long continued stream of the gas was therefore passed through

a series of Woulfe's bottles, the first containing lime-water, the second nitrous acid, the third water, and the fourth dilute solution of sulphate of iron: from the last bottle proceeded a tube which plunged deeply in the water-cistern. At the end of the process the first bottle contained carbonate and hydro-sulphuret of lime; in the second the nitrous acid was rendered colourless; the water of the third prevented the passage of nitrous acid vapour into the solution of sulphate of iron; and this solution in the fourth detained a quantity of nitrous gas, as was shown by its olive colour. The hydrogen which came over when agitated with water, was destitute of smell. When a jar of it was kindled, it burned gradually, but the light was so faint and rare that it had no colour. I was not prepared to take the specific gravity of this purified gas.

The nitrous acid did not manifest the least traces of arsenic or phosphoric acid. When evaporated to dryness, it left a portion of matter too minute to ascertain the nature of.

From the foregoing it appears that, in order to obtain pure hydrogen, we must first agitate common hydrogen with lime-water during a few minutes, next with a little nitrous acid, afterwards with dilute solution of sulphate of iron; and lastly, with water. The gas is now entirely deprived of smell.

For this purpose, a funnel the throat of which is fitted with a ground glass rod, its pipe being ground to fit the mouth of a large bottle, will be a convenient apparatus. When the fluids are to be poured out, it must be done by inverting the bottle under water, and withdrawing the funnel.

XXXVII. *Notices respecting New Books.*

Mr. Accum has in the press "A practical Essay on Chemical Reagents or Tests, illustrated by a Series of Experiments."

The work will comprehend a summary view of the general nature of chemical tests;—the effects which are produced by the action of these bodies;—the particular uses to which they may be applied in the pursuits of chemical science;—and the art of applying them successfully. A List of those substances for which there exist any appropriate tests will be added; and a portable chemical chest, containing all the chemical tests and apparatus necessary for performing the experiments described in this Treatise, may also be had with the work, which will be published early in September,

The Rev. T. Maurice, the learned author of Indian Antiquities, has in the press "Observations on the Ruins of Babylon."

Mr. Maurice

Mr. Maurice intends in the course of the work to prove that the famed Tower of Babel was a Temple to the Sun, and the whole of that vast city was constructed upon an astronomical basis—showing also the high advance of the ancient race of Fire-worshippers (its founders) in metallurgic science, in architectural design, in geometry, in mechanics, in hydraulics, in the art of engraving, colouring, &c. Together with strictures on the Babylonian bricks, and their inscriptions, preserved in the British Museum—On the ruins of Persepolis, or Chelminar; including a dissertation on a lately discovered Persepolitan monument, of high importance to astronomers, and supposed to contain a portion of the ancient BABYLONIAN SPHERE—On the presumed Antiquity of the Arch, no where to he found amid these ruins—On the Origin of Alphabetic Writing, and various other subjects connected with the history of the most ancient periods. With illustrative engravings.

The work will be published by subscription. Price one guinea.

Subscriptions received by the Author at the British Museum; also by Mr. Murray, Albemarle-street; and Mr. Richardson, 23, Cornhill.

Mr. Ryan, who lately obtained the prize of 100 guineas with the gold medal from the Society of Arts, for his new system of ventilating coal-mines, has in the press A Treatise on Mining and Ventilation, and embracing in a particular manner the subject of the Coal Stratification of Great Britain and Ireland; with the most approved methods of discovering, working, and ventilating the same.

Mr. Forster is collecting subjects for a work he intends to publish, On the generic Forms of the Crania in Animals.

· A new Edition of the Rev. Mr. Harmer's Observations on various Passages of Scripture, with many important additions and corrections, by Adam Clarke, LL.D. F.S.A., will be published in a few days, elegantly printed in four octavo volumes.

XXXVIII. *Proceedings of Learned Societies.*

ROYAL MEDICAL SOCIETY OF EDINBURGH.

THIS Society has proposed the following as a Prize-question: "What are the chemical changes produced in the air by the growth of plants? and do they on the whole purify or deteriorate the atmosphere?"

A set

A set of books, or a medal of five guineas value, will be given annually to the author of the best dissertation on an experimental subject proposed by the Society; for which all the members, honorary, extraordinary, and ordinary, will alone be invited as candidates.

The dissertation ⬛⬛⬛⬛ written in English, French, or Latin, and to be ⬛⬛⬛⬛ the Secretary on or before the 1st of December of the ⬛⬛⬛ year to that in which the subjects are proposed ; ⬛⬛⬛ adjudication of the prize will take place in February following.

To each dissertation must be prefixed a motto ; and this motto is to be written on the outside of a sealed packet containing the name and address of the author. No dissertation will be received with the author's name affixed ; and all dissertations, except the successful one, will be returned, if desired, with the sealed packet unopened.

KIRWANIAN SOCIETY OF DUBLIN.

Dec. 13, 1815. A paper was read by D. Wilson, Esq. " On some Liquid Combinations of the Oxymuriatic Acid, and their Application to the discharging of Turkey Red in Calico-printing."

The Turkey red is not discharged by any of the oxymuriates hitherto employed, but for this purpose oxymuriatic acid has been used. This produces many inconveniences. It is highly deleterious to the workmen. By acting on the metals of the press employed in printing with it, black spots are occasioned in the stuff, and its texture is injured. Mr. Wilson found the discharging power of the oxymuriates to be in proportion to the weakness of affinity between their constituents. Hence he found oxymuriate of alumina to answer the purpose, even in a shorter time than the acid, and without any of its inconveniences. It has been since pretty extensively used. He prepares it by decomposing clear solution of oxymuriate of lime with solution of alum. In the same manner he recommends various other oxymuriates to be prepared for the purposes of the arts, and points out their application.

May 15, 1816. A paper by M. Donovan, Esq., Secretary, was read, " On the Impurities of Hydrogen as it is commonly obtained," and the experiments were repeated before the Society. It is unnecessary to give any abstract of this paper, as it is inserted in full in the present number.

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XXXIX. *Intelligence and Miscellaneous Articles.*

AERIAL NAVIGATION.

IN consequence of Sir George Cayley's communication, inserted in our number for May, we have been favoured with a letter from a gentleman of distinguished literary and scientific reputation. He is " inclined to believe that something useful to mankind on the subject may be either positively or negatively ascertained, under the conduct of men of science." He is willing to contribute fifty pounds for furthering any plan which may obtain the sanction of a few men of the description to which he alludes.

Sir George Cayley has requested us to correct an error in the title prefixed to his last article, in which F.R.S. was affixed to his name by mistake. The fact was simply this: The Editor, who was obliged to leave town for a few days, sent the communication without any title, along with some other articles to the printer to put to press in his absence ; and trusting to memory instead of looking at Sir George's previous communication, the printer committed the error complained of.

CORRESPONDENCE OF M. VAN MONS, CONTINUED*.

Brussels, July 1816.

I have of late paid particular attention to the manner in which the solutions of salts are decomposed by reduced metals. We see a liquor thicker than water, separated from the solution and approach the metal: then it disappears, and at the same instant the reduced metal shows itself in a state as if it had undergone fusion. It is not to be supposed that the acid quits for some time the oxide, to admit of the latter taking up the oxygen by the decomposing metal ; for this would be granting too much ; and besides, the solution reacts at no period on the blue colour with which we stain it. We ought rather to conceive that the

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* In the translation of the letter of M. Van Mons, given in our number for March, pp. 214 and 215, the muriate of lead of Derbyshire is said to consist of lead and " *phosphoric acid;*" the latter ought to have been " *phosgen acid.*"

water

water of the solution, displaces from the decomposing metal hydrogen, which approaches the decomposed salt, to place itself in the room of its equivalent under the form of water, and peculiarly to the water of oxidation of the oxide of this salt, whereby this oxide is reduced, and that the acid adhering to this water is substituted for the hydrogen, or takes ▓▓▓ of this principle, and thus forms in every respect th▓ ▓ It is impossible that the case can be otherwise. ▓ not fused, but it is newly composed. It is the acti▓ ▓kes place under the pile, and wherever oxides are re▓ ▓thout their oxygen being hot enough to pass to another bo▓ When the solution is saturated with salt, the decomposition is slow or null: this is because the metal precipitated has not a free motion: but is this requisite? I say no: but free water is requisite for it, and particularly water of solution which is separated from the salt; then it requiries the caloric with which this water is supercomposed. The metals which ▓re oxidated by water are not separated or reduced, and wh▓ ▓ey are so they form no arborescence.

It has been discovered in Italy, that the tincture of salt of tartar dissolves phosphorus without decomposing it, or forms a liquid phosphuret of its alkali: and caustic lixivium diluted in alcohol operates the same solution. Here we have in the first place the defect of water, and afterwards the affinity of water for alcohol, which hinders the phosphuret from being decomposed as soon as it is formed. But add more water, and particularly warm water, and instantly phosphuretted hydrogen gas appears. The alcohol contracts a very intimate union with the strong alkalies, and the alcoholate of potash, if not the potassate of alcohol; for I think that in this union the alcohol acts like an oxide, and the potash like an acid, and becomes very intensely hydrated. I once found large crystals in a very old tincture of salt of tartar: they were neuter on the vegetable blues: to the last they were at first salt and bitter, and afterwards very corrosive. In the open air, when I wished to dry them they deliquesced, giving out a smell of alcohol, and nothing▓t caustic potash remained. The little difference between the intensity of the alcohol and of the potash, makes this salt be so easily decomposed, but it is hydrated in the ratio of the combined energy of the elements. Its hydrate is already liquid. I have thus obtained in a crystalline form oxygenate of potash. This salt was furnished to me by treating caustic potash with red oxide of mercury.

I have succeeded in depriving ink of its principle of corruption by infusing the gall-nuts in common beer vinegar. They are to be broken into a coarse powder, and infused two or three days in

a retort closed with a piece of paper. The infusion is then passed through a woollen sieve; the residue washed in cold water, decanting all that remains suspended in the water; the latter portion is then to be infused in the same manner in pure water, and both infusions are to be mixed. The whole is to be heated for an instant, and then allowed to subside for twenty-four hours, when it is to be filtered again : gum and sugar are then to be added ; and when they are dissolved, the whole is to be once more passed through the sieve. The ink is then to be mixed with the oxide or red sulphate, but neither the acidulated nor oxidulated sulphate ought to be used. The whole being then shaken, may be put into a stone bottle and corked with a paper stopper.

Not only is the ink thus prevented from being corrupted, but it loses another bad property, namely, that of thickening. The acid of the vinegar is combined as a mucous kind of acetate with the mucilaginous matter, and precipitates it; the vinegar is very much softened in this infusion by the connexion which its acid parts form with the mucus. The thickening of the ink arises from the sulphuric acid rendered free, precipitating this body. The mucus of the gum arabic scarcely undergoes this change at all. I consider myself fortunate in having so well succeeded in this preparation.

I lately made a very singular experiment. I was directing on some red oxide of mercury scarcely heated to 30° of Reaumur, a jet of hydrogen gas from a bladder. I wished to obtain water, but did not obtain any, but the red oxide became white. I heated it more fiercely, and it was red hot before the steam of the water appeared; and then instead of reduced mercury there remained black oxidulate, which. I had much difficulty in de-oxidating. I forgot to say, that during the process the matter instead of becoming of a dull purple colour became yellow only.

Having repeated the experiment at a low red heat, I obtained water and concrete reduced mercury which resisted the fire a long time, giving out hydrogen and liquid metal. I operated with a double crucible, the upper one being of glass. My first idea of this phænomenon was incorrect; for I had, like Dobereiner and Davy, made of the mercury a new metal concrete even in the fire, by incorporating an overplus of hydrogen at first with its oxide, and afterwards with the reduced metal.

· Hence came a metal more intense, and which will not be more deoxidable in the fire, if it does not remain decomposable in the hydrogen added. We may regard this super-hydrogenation, when it takes place upon the oxide, as a mineral organization, and the same with that which carbon undergoes in plants, and

<div align="right">azote</div>

azote in air and animals. The irreducible silver which Richter procured from an alchemist was silver organized by water, and thus become a new metal, more intense on acc)art of its oxidation by water instead of oxygen, and without depositing or separating hydrogen. Tellurium and arsenic are organized by water, and converted either into new metals, or into kinds of acidifiable combustibles, in hydrogenated bodies and in the tellurated and arsenicated hydrogens. I ought to have reflected that in the first experiment I had only obtained mercury, oxygen and water: this first experiment was so far particular, that in order to be reduced the mercury became suboxidated, which in common cases does not take place; but it is true that the second oxygen was here taken up by the hydrogen. The new metal does not appear susceptible of any but one degree of oxidation.

You must doubtless have perceived that the double acidifiable combustible which with hydrogen forms prussic acid, and which scientifically ought to be called Prussium, (hence prussiated hydrogen gas, prussiate, prussure, &c.) is with respe t to azote what the alcohol of Lampadius (and which we might also without much impropriety call lampadium, and hence lampadiated hydrogen gas, lampadotes, lampadures, &c not to say sulphuret and azoturet of carbon, which would be false,) is with respect to sulphur. The two compounds have the same physical and chemical properties: they are very gasifiable, colourless, and diaphanous; dry oxygen cannot resolve them into the acids of their elements, and without water we could not decompose them. We see, in short, that one of the combustibles acts with respect to the other, in the room of water; so that this last is not only reduced, but subtracted in the portion of hydrogen which composes its oxygen into water. If therefore one of the compounds is the alcohol of sulphur, the other is alcohol of azote; and in both, in their solution by hydrogen, the carbon goes for the first proportion of this principle; and the one ought already to be considered as hydrogenated sulphur, and the other as azote hydrogenated, but by carbon instead of hydrogen.

You must know that iodine receives reduced metals in exchange for its oxygen, and forms, like the dry fluoric and muriatic acids, combustibles salifiable by oxygenation; for instance, by the oxidation of their metals. The hydrogen does not displace the metal from them, but composes the metallo-iode into iodure of reduced metal, by producing actually acidifiable combustible from the iodic acid.

The oxygenated iodine which Sir Humphry Davy obtained by treating iodine with oxygenated chlorine, is not eu-iodine, but

sub-euiodine, or iodine simply oxygenated, the euiodine being considered as hyperoxygenated. The combinations which this body contracts with the acids are analogous to those which it contracts with water. It is probable that the light will separate the oxygen from it, as well from the sub-euiodine as from the iodine, and with these two acids as it were with analogous engagements to the oxido-boric, carbonico-muriatic gases, &c. These compounds are very curious. What has been hitherto regarded as insulated euchlorine, is in the same way only oxygenated chlorine, the euchlorine being hyperoxygenated chlorine. The dry acid in this compound is at all times free enough, by its subsolution by the acid of the oxygen, to unite with the oxidulate of mercury, without adding to its degree of oxidation.

I am informed that Sir Humphry Davy has undertaken a laborious inquiry into the de-hydrogenation of the fluoric acid into fluorine, availing himself of iodine with this view. I shall venture to predict that his researches will be fruitless. I have made many experiments with the same view, but I never perceived the slightest prospect of succeeding. The fluoric acid is not oxygenable into fluorine, and it is not perhaps any more hydrogenable into fluor ; but it will rather be the latter than the former, since already it prefers the most feeble reduced metals to those most strongly oxidated. Gold, platina, &c. separate it from lime : it is upon those compounds of dry fluoric acid and upon reduced metals, or upon the metallo-fluors, that all attempts must be made to dehydrogenate this acid. But according to my first results, there are formed *fluures* very much hypercomposed of metal, and from which the fluor does not appear separable. We must find out a metal not susceptible of *fluurisation*, and it would be fortunate if we could find it among the volatile metals, at least if the fluor be not volatile, and then act on the metallo-fluor of this metal, made red hot by means of a stream of hydrogen gas. It is singular that the hydrargyro-fluor is not, any more than the two hydrargyro-mures, or hydrargyro-chlores, volatile in the fire. If there existed a metal nearly inoxidable, we might expect that its *fluoric* would be given up to the oxygen, the metal remaining untouched, and then the fluorine will be produced. With the fluors of ordinary metals, the metal is oxidated, and dry fluate results from its combination with the *fluoric*. I call *fluoric* the dry fluoric acid, or fluoric acid gas without any water; *fluorine* when it is freed from oxygen ; and *fluore* when freed from hydrogen; and *dry fluates* when freed from their oxides. This dry acid has no insulated existence, any more than the muriatic, iodic, sulphuric, azotic, &c. It is a
                                                                    body

body naturally less burning (*comburant*) than oxygen, which is the element of all *comburation*, but more burning (*comburant*) than water, since it burns reduced metals, and since water serves it for an oxide, in the ordinary acids, which on that account I have named fluate, muriate, sulphate, &c. of *hydrose*, not to say of water.

I have made several additional experiments of late, which have confirmed my opinion, that the ashes of the metals are acids or oxidules more or less combined with reduced metal, which makes it necessary for the acids to be heated, in order to dissolve them, or in order that the oxidated part may be separated from the reduced part, and that they should share it in those two parts in the solution, which without caloric could not take place if the oxide be not already in existence. The combinations between the oxidules and the oxides, and between the latter and the hyperoxides, form the degrees called intermediate of oxidation, and which intermediate degrees some authors have not yet ceased to admit. I call hyper-oxide every oxide which ought to deposit oxygen, in order to be capable of being dissolved in the acids, and which consequently gives chlorine with muriatic acid. Several of the organic substances are combinations between oxides and oxidules; and as the affinity is weak, both the one and the other is easily engaged in excess; and when we analyse these substances, we frequently do nothing more than combine them with the reagents; and then, instead of *educts*, we have *products* which we mistake for new principles. The fine analysis of the oxalates by fire has proved, what I always said, that the vegetable acids have for their common fixed base carbonous acid, and for a varying principle water. This analysis has yielded out of the oxalates with weak metals, carbonic acid and metal reduced after having allowed the escape of water. There are carbonites therefore. The same salts with stronger oxides have yielded water, and the carbonites remaining have been resolved into gaseous oxide of carbon; and the oxalates with very strong metals are decomposed, after the manner of the acids alone, into water, acetous acid, carbonic acid, oil, carbonated hydrogen gas, with hydrogen and carbon as the residue.

Not only is the residue of the sulphuric ether an acidimulated sulphate of ether, but also, what is singular for an acid which changes its own water with the oxides, and forms dry salts, that of the ether by the fluoric acid is an acidimulated fluate of ether, which in these salts exists in the state of gas, and the excess of acid is therein hydrated. From the new alcohol, neutral compounds are made, which the fire once more resolves into insulated ethers, and into acidimulated salts. These salts are not decomposed

posed by the alkalies; but, by saturating the excess of acid, they so act that the sulphate or fluate of ether, become neuter, may still be divided into isolated ether and acidinulated salt. The stronger acids assume with the oxides of any of these neutral or acidinulated salts the place of weaker acids. Ether is not separable from the acidinulated salts, but by its destruction. Boulay has confirmed that the ethers are sulphydrated alcohol: I say confirmed, because I said so a long time ago. The carbon and hydrogen are there in the same absolute ratio, but less organized by water. It is not astonishing that an oxide so energetic as the ethereous gas contracts with the acids equally strong engagements. In short, when we say " there is in chemistry nothing but hydrogen, metals, oxygen, combustibles and acidifiable *comburans*, oxides, acids, and salts," we have embraced the whole science, simplified the course of things, and placed as it were an astronomical station.

<div align="center">[To be continued.]</div>

<div align="center">EARTHQUAKE IN SCOTLAND.</div>

About eleven o'clock on the night of the 13th of August a tremendous shock of an earthquake was felt in various parts of the north of Scotland. At Aberdeen, Perth, Montrose, and Inverness, its effects were most remarkable; but although many houses in all these places were shaken from their foundations and partially shattered, we are happy to add, that from the durable and massy architecture of the houses in Scotland, no human lives were lost. Several bridges in the district thus visited also suffered severely, but the most singular phænomenon attending the awful concussion is the effect which it produced on the spire at Inverness. A letter from this place thus describes part of the devastation there in the following terms:

" Chimney-tops were thrown down or damaged in every quarter of the town. The Mason Lodge, occupied as an hotel, was rent from top to bottom, the north a stalk of the chimney partly thrown down; one of the coping-stones, weighing, I should think, from fifty to sixty pounds, was thrown to the other side of the street, a distance not less than sixty feet. The spire of the steeple, which I think one of the handsomest in Scotland, has been seriously injured, and must in part be taken down. The spire is an octagon; *and within five or six feet of the top, the angles of the octagon are turned nearly to the middle of the flat sides of the octagon immediately under it.* What is more wonderful than any thing attending the memorable event, notwithstanding the vast quantities of stones and bricks that have been thrown from such immense heights, not one person has received any hurt!"

INTENSE HEAT.

Our chemical and mineralogical readers will be glad to hear, that by means of a blow-pipe for burning the gaseous constituents of water, in a state of high condensation, and which is constructed by Mr. Newman, of Lisle-street, Leicester-square, a degree of heat may be produced greater than that of the most powerful Galvanic battery. Professor Clarke, of Cambridge, who is engaged in a series of analytical experiments with the blow-pipe, has already succeeded in the decomposition of the earths; having obtained metals from barytes and strontian, which do not become oxided by exposure to atmospheric air. The metal of barytes is ductile, and has the lustre of silver. An account of these experiments will be shortly before the public.

A correspondent in Sussex has remarked the very rapid and great evaporation from the earth's surface during the clear intervals of the late showery weather,—a circumstance which may account in some measure for the continuance of so much rain.

STEAM ENGINES IN CORNWALL.

By Messrs. Leans' Report for July, the average work of twenty-seven engines was 20,142,363 pounds lifted one foot high with each bushel of coals consumed.

During the same month the work performed by Woolf's engine at Wheal Vor was 47,610,798 pounds lifted one foot high with each bushel. His engine at Wheal Abraham that is in full work, lifted during the same month 51,923,679 pounds one foot with each bushel. His other engine at the same mine, not yet at her full load, lifted 23,794,469 pounds to the same height with every bushel of coals.

The last mentioned engine has a 60-inch cylinder, but is only loaded 2 lib. 9 per square inch ; while his other engine at the same mine (with a 45-inch cylinder) has a load of 15 lib. 1 per square inch ; that is, a great portion of the coals consumed with the 60-inch cylinder engine goes only to the motion of the engine.

MAJOR PEDDIE'S EXPEDITION TO AFRICA.

The spring transport which carried Major Peddie and his companion Capt. Campbell to Africa, arrived after a tedious passage at Goree ; but the death of the surgeon who was to have accompanied them, which took place on the 8th of December at Senegal ; and the troops which were to have arrived from Sierra Leone in December not joining till the 25th of February, unavoidably delayed the department of the expedition from the coast till too late for the season. This delay will, however, be

K 4   attended

attended with advantages, as it will enable Capt. Campbell, who will make the necessary astronomical observations, to settle the position of many places on the coast of Africa with more precision than has hitherto been done. He had in the month of February last made a great number of observations of distances of the sun and moon, and moon and stars, from which he found the longitude of Several different from what is given in the tables; the latitude Capt. Campbell fixes from his observations at 16° 2′ 30″ N. He was, however, anxious to avail himself of a greater number of observations before he finally fixed the longitude of the town; which as soon as he has done, he promises to transmit for the use of navigators. The expedition has been fitted out with several good sextants, principally of Mr. Carey's make, and two of them contain a contrivance for taking single altitudes by means of a level contrived by the late Lieut.-gen. Brown, under whose management the astronomical part of the expedition was fitted out. Besides sex , Capt. Campbell has barometers (principally Mr. Arnold's) a small transit instrument; but the latter is so badly made, that the captain says, in a letter to his friend in England, he cannot make any use of it. As this expedition was planned rather late in the spring of last year, and great fear was entertained of losing the season, the astronomical apparatus was as well as many other parts hurried. Mr. Troughton would no doubt have been employed; but the shortness of the time, and the engagements of this excellent artist, deprived the expedition of the benefit of his superior instruments; and although this circumstance has occasioned some regret to the astronomer employed, he has however great satisfaction in believing, with the exception of the transit *, all the other instruments are very well executed. It was perhaps impossible for Government to have selected two gentlemen better qualified to undertake this arduous enterprise; and from the care which has been taken in the equipment, the public may look forward with great probability to the full accomplishment of the object of the expedition.

RECIPES FOR MAKING JAPAN OR VARNISH.

The following recipes for making japan or varnish are given in a contemporary journal, as having been recently imported from Germany. These compositions are described as rendering all articles to which they are applied impervious to wet without destroying elasticity.

White Japan.—This japan never changes its colour, and with-

* The transit instrument was not of Mr. Carey's making. Mr. Carey attempted to improve it; but its original bad construction, it seems, would not admit of it.

stands all the chemical agents that blacken other white pigments used in japanning. It is obtained in the following manner:

Let some artificial carbonate of barytes (obtained by decomposing or pouring into a solution of native carbonate of barytes a saturated solution of subcarbonate of ammonia) be ground up with a sufficient quantity of white oil varnish, and apply it successively upon the leather. This being done the finishing coats are given to the article with a japan composed of carbonate of barytes ground up with white copal varnish; and when perfectly dry, the leather is polished with a piece of felt and finely levigated pumice-stone powder, and the last or finishing polish is applied by means of a sponge or soft brush and burnt hartshorn powder.

Yellow Japan.—To obtain a clear transparent yellow, the leather must of course be white, and a yellow dye is given to it by means of wood or French berries and alum; and when perfectly dry the japan ground of patent yellow is applied in the manner above stated.

Red Japan.—For this purpose the base of the japan ground must be made up with madder lake ground up with oil of turpentine; this forms the first ground. When perfectly dry, a second coat must be applied, composed of lake and white copal varnish; and the last, with a coat composed of a mixture of copal and turpentine varnish ground up with lake.

Blue Japan.—The first coat must be given with artificial carbonate of barytes ground up with oil varnish; the second with prussian blue, ground in copal varnish and finished as before stated.

Black Japan is obtained by applying finely levigated ivory black ground up with linseed oil varnish; the second coat must consist of the same pigment ground up in copal varnish.

EDINBURGH DISSECTION OF THE BRAIN.

A considerable interest having been excited in Edinburgh for some time past, about the discoveries of Gall and Spurzheim, relative to the structure and functions of the brain, Dr. Spurzheim at length made a dissection of that organ in one of the anatomical dissecting rooms of the University. Besides the regular students many of the professors were present, as well as other scientific persons interested in these new and important discoveries. Dr. S. succeeded in making the most perfect dissection of the brain, and received the approbation of those who were present. Many persons who had previously opposed the new method of dissecting this organ, testified to the superiority of Dr. Spurzheim's new mode of developing the hitherto unexplored structure of the brain and nervous system.

NEW NAUTICAL INSTRUMENT.

To Mr. Tilloch.

Sir,—In order that the instrument offered to your notice may become as useful to mariners and travellers as the problem on which it is grounded was to the inventor and his shipmates in the years 1813 and 1814, he respectfully so.......d publication of this in your very useful Magazine.

The instrument alluded to is called a Nautic Indicator, and is a representation of the circles and arches of the natural hemisphere, as far as they are necessary to the seaman and traveller, and when set to observe altitudes of the sun, or of a star, at any time of the day or night, gives a distinct view of the meridian altitude; zenith distance; azimuth; amplitude; true time; length of the day, and variation of the needle at the place of observation, without the possibility of (unwilful) error; by placing the whole under the eye of the observer in the natural positions.

To a traveller by land, no other accompanying instrument than a spirit level is necessary, to ascertain all the abovementioned articles. At a time when perhaps the mind is too much agitated for numerical calculation, it will in two or three minutes remove every uncertainty with respect to place at one observation.

To the curious in his study, it will readily solve many important problems in both geography and astronomy as well as navigation, among which the progressive variation of the magnetic needle is not the least.

To the teacher in his school, this instrument will be of much benefit, in detecting the errors of his pupils, and giving them a clear demonstration—that, with the observed altitudes, the interval of time between each, and the known declination at that time, the sun is precisely at the same distance from the meridian, and that the meridian altitude can be no other than that given by the instrument, as also all the forementioned objects indispensably necessary to navigators.

To merchants this invention must be of the utmost consequence, since by death, sickness, or casualties, the charge of their ships and cargoes often devolves on those whose knowledge, through want of proper education, is far from being adequate to the trust.—The instrument is of the most simple construction, and the application may be understood by almost any capacity in a few hours—by a mariner at first sight.

Any gentleman willing to patronize this invention, will please to address to James Hunter, No. 9, Leicester-street, Leicester-square, London; or at Mr. John Thin's, architect and builder, Edinburgh.

DISEASES

DISEASES OF DOGS.

In a work recently published, entitled "Instructions to young Sportsmen," by P. Hawker, Esq. we find the following recipes for treating the diseases and accidents to which these useful animals are subject. The author's long and extensive experience enables him to speak with confidence on such topics, and we have no doubt he shall confer a benefit on society at large, by making public the means which he prescribes.

"*Distemper.*—To enumerate the various recipes for this *sometimes incurable* disease would require a volume; but, of all that I have yet tried, none has answered better than the one I shall here give; and, as the remedy is so innocent, it may be safely administered where there exists even a doubt as to a dog having the distemper.

"*Recipe*.*

"Opium . . . . 3 grains.
Emetic tartar (an invaluable medicine) . . 5 grains.
To be given at night.

"Repeat the dose, every third night, till the dog is recovered; taking care to keep him in a warm place, and always fed with a warm liquid diet, such as broth, gruel, &c.

"If the nostrils should discharge, have them washed, or syringed, twice a day, with a lotion of alum, or sugar of lead; putting about half an ounce of either to a pint of water.

"The following is a recipe, which no bribe could tempt the vender to part with; but, by means of some very clever chemists, I have ascertained it to be simply as follows; after some trouble in discovering the proportions, and discarding the ingredients by means of which it was disguised in a pill.

"*Recipe.—For a half-grown Pointer.*
"Jalap powder 25 grains. Calomel 5 grains.
Made into a pill with a little gum water.
"*For a full-grown Pointer.*
"Jalap powder 30 grains. Calomel 8 grains.
Mixed as above.

"One of these doses, mixed with butter, or in a small piece of meat, should be given to the dog every morning, on an empty stomach. The food should be light, and easy to digest; and the lotion, if required for the nostrils, should be observed here, as before mentioned.

"Notwithstanding the trouble we had to discover this simple recipe, I should prefer the *one first given*, because there is less chance of a dog *taking cold* with that, than any kind of *mercurial* preparation."

* "The following prescriptions are each about a dose for a full-grown pointer. They must, of course, be increased or diminished in proportion to the size and strength of the dog."

LECTURES.

Royal Institution.—Mr. Brande will commence his extended and practical Course of Lectures and Demonstrations on Chemistry, on Tuesday the 1st of October, at Nine in the Morning. The days of lecturing are Tuesdays, Thursdays, and Saturdays.

London Hospital.—On Tuesday the ⬛⬛⬛ October, Mr. Richard Phillips will commence a Cour⬛⬛⬛⬛res on Chemistry, at Half past Seven o'clock in the Evening, to be continued every Tuesday, Thursday, and Friday. Two Courses will be given in the Season, which commences in October, and terminates in May. Further Particulars may be had of Mr. W. Phillips, George-yard, Lombard-street, and of Mr. Jenkinson, Apothecary, London Hospital.

Medical School of St. Thomas's and Guy's Hospitals.—The Winter Course of Lectures at these adj⬛⬛⬛ Hospitals will commence as usual the first of October; vi⬛⬛⬛

At St. Thomas's.—Anatomy and the Operations of Surgery, by Mr. Astley Cooper and Mr. Henry Cline.—Principles and Practice of Surgery, by Mr. Astley Cooper.

At Guy's.—Practice of Medicine, by Dr. Babington and Dr. Curry.—Chemistry, by Dr. Babington, Dr. Marcet and Mr. Allen.—Experimental Philosophy, by Mr. Allen.—Theory of Medicine, and Materia Medica, by Dr. Curry and Dr. Cholmeley.—Midwifery, and Diseases of Women and Children, by Dr. Haighton.—Physiology, or Laws of the Animal Œconomy, by Dr. Haighton.

N. B. These several Lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a Complete Course of Medical and Chirurgical Instruction. Terms and other particulars may be learnt from Mr. Stocker, Apothecary to Guy's Hospital.

St. George's Medical, Chemical, and Chirurgical Schools.—The Courses will commence the first week in October:

1. On the Laws of the Animal Œconomy and the Practice of Physic, by George Pearson, M.D. F.R.S. Sen. Physician to St. George's Hospital, &c. &c.

2. On Therapeutics with Materia Medica and Medical Jurisprudence, by George Pearson, M.D. and W. T. Brande, F.R.S., Professor at the Royal Institution.

3. On Chemistry, by W. T. Brande, F.R.S. Professor of Chemistry at the Royal Institution.

4. On the Theory and Practice of Surgery, by B. C. Brodie, F.R.S. Assistant Surgeon to St. George's Hospital, &c.

Note.—Sir E. Home will continue to give Lectures on Surgery gratuitously to the Pupils of St. George's Hospital.

Dr. Clutterbuck will begin his Autumn Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Wednesday, October the Second, at Ten o'clock in the Morning, at his House, No. 1, in the Crescent, New Bridge-Street, where further Particulars may be had.

Theatre of ————Mr. Taunton's Lectures on Anatomy, Physiology, ————and Surgery. The Autumnal Course will commence on Saturday, October 5, 1816, at Eight o'clock in the Evening *precisely*, and be continued every Tuesday, Thursday, and Saturday, at the same hour.

Particulars may be had on applying to Mr. Taunton, 87, Hatton Garden.

Theatre of Anatomy, Medicine, &c.   Blenheim Street, Great Marlborough Street.—The Autumnal Course of Lectures at this School will begin on the following Days :

Anatomy, Physiology, and Surgery, by Mr. Brookes daily at Two, on Tuesday, October 1, 1816.   Dissections as usual.

Chemistry and Materia Medica, &c. daily at Eight in the Morning.

Theory and Practice of Physic at Nine, with Examinations, by Dr. Ager, on Monday, October 7.

Three Courses are given every year, each occupying nearly four months.   Further particulars may be known from Mr. Brookes, at the Theatre; or from Dr. Ager, 69 Margaret Street, Cavendish Square.

Meteorological Observations kept at Walthamstow from July 14 *to August* 14, 1816.

[Between the Hours of Seven and Nine A.M.]

Hour. Therm. Barom. Wind.

July

14	62	29·90	S.—Cloudy and sun ; rainy afternoon.
15	59	29·60	S.—Rain ;—showers and sun. Evening clear;—*cirrostratus* NW.
16	59	29·60	E. var.—Very rainy day and evening.
17	58	29·53	NW. var.—Clouds and wind; sun and showers; clear. Moon last quarter.
18	52	29·40	S. var.—Sun and clouds; showers and sun ; clear and clouds.
19	55	29·40	SE.—Very rainy until after 6 P.M.; cloudy.
20	63	29·70	S.—Clear and clouds, *cirrostratus;* hot sun, and wind ; at 8 P.M. remarkable *cirrocumuli* NW.; clear and clouds.

July 21.

Hour. Therm. Barom. Wind.

July

21	65	29·45	S.—Showery all day; clear and clouds.
22	57	29·70	SE.—Hazy; fine day; showers; clear and clouds.
23	60	29·60	SE.—Showery day.
24	56	29·61	NE.—Clear and cloudy; great darkness at noon; clear and clouds; hazy; new moon.
25	60	29·60	SE—N.—Showery; clear and clouds.
26	56	29·70	NW—S.—Sleet showers; sun and wind; clear and clouds.
27	59	29·88	S—SE.—Clouds and sun; showery.
28	56	29·81	NW.—Sun and wind; drops of rain; starlight.
29	59	29·70	N.—Fair morning; slight showers; clear and clouds.
30	59	29·70	N.—Sun and clouds; showers; star-light.
31	48	29·56	W.—Sunshine and hazy; fine day; clear and clouds.

August

1	59	29·50	N.—Clear and clouds; fair day; small drops of rain; clear and clouds.
2	51	29·80	S.—Sun; showers; clear and clouds.
3	51	29·82	S.—Sun; fine day; moon-light; *cirrostratus* N. Moon first quarter.
4	58	29·90	S.—Sunshine; very fine day; moon and stars.
5	66	29·75	E.—Hazy; clear and clouds; sun; cloudy and dark.
6	53	29·90	NW.—Sunshine; sun; showery; rain.
7	62	29·90	SE.—Rain; hot sun; showery.
8	63	29·80	S.—Sun and clouds; fine day; shower at 7 P.M.
9	59	29·72	S.—Sun; showers; cloudy.
10	55	29·91	NW.—Gray; fine day; clear and clouds; Moon full.
11	60	30·00	SE.—Clouds and sun; small showers; fine day; cloudy.
12	59	30·00	NW—S.—Cloudy; showers and some sun; clear and clouds; stars and moon.
13	55	30·00	NW.—Clear and clouds; very fine day; star-light.
14	54	29·80	NW.—Gray; showery; cloudy.

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1816.	Age of the Moon.	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
July 16	21	66·	29·80	Fair
17	22	62·	29 60	Fair
18	23	61·	29·54	Showery
19	24	60·5	29·52	Rain
20	25	67·	29·81	Showery
21	26	68·5	29 63	Fine morning—P.M. showery, and gales from S.
22	27	63·	29·78	Rain
23	28	66·	29·65	Rain
24	new	63·	29·63	Rain
25	1	60·5	29·74	Rain
26	2	63·	29·95	Fair
27	3	61·	30·	Fair
28	4	57·5	29·82	Showery
29	5	59·	29·70	Fair
30	6	59·	29·60	Fine till noon—a violent storm of hail and rain at 1 P.M.
31	7	60·	29·62	Fair
Aug. 1	8	60·	29·68	Rain
2	9	59·	29·85	Fair, but cloudy
3	10	61·	29·83	Fair
4	11	60.	29·90	Fair
5	12	62·	29·90	Very fine till evening, then rained violently
6	13	62·	30·	Showery
7	14	66·	29·90	Fair till noon—then showery
8	full	65·5	29·80	Rain
9	16	63·	29·82	Showery
10	17	62·	30·10	Fair
11	18	62·	30·02	Showery
12	19	63·	30·12	Fair
13	20	68.5	30·07	Fine
14	21	63·	29·85	Showery

There has hardly been a day for the last month, in which it has not rained more or less towards the evening.

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For *August* 1816.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.			
July 27	58	66	56	29·92	47	Fair
28	56	65	52	·73	42	Cloudy
29	54	60	54	·58	45	Showery
30	54	61	52	.50	40	Showery
31	52	64	54	·43	49	Fair
Aug. 1	54	66	55	·60		Fair
2	55	64	55	·80		Showery
3	54	69	55	·83	45	Fair
4	56	70	57	·86	51	Fair
5	57	67	56	·90	48	Showery
6	56	68	57	·91	40	Cloudy
7	57	67	56	·85	36	Showery
8	56	70	57	·73	55	Fair
9	56	66	56	·74	50	Showery
10	57	67	55	30·00	57	Fair
11	58	68	56	·02	49	Cloudy
12	57	66	55	·03	50	Showery
13	59	64	56	29·88	42	Showery
14	58	65	57	·76	40	Showery
15	57	66	56	·55	36	Showery
16	57	65	56	·75	46	Fair
17	56	61	55	·80	37	Showery
18	54	66	55	·99	46	Fair
19	55	68	56	30·13	54	Fair
20	57	60	50	·10	50	Showery
21	52	63	56	·20	55	Fair
22	56	64	57	·14	40	Cloudy
23	57	63	57	·12	36	Cloudy
24	57	65	50	·13	46	Cloudy
25	50	66	55	·22	54	Fair
26	52	63	52	·18	50	Fair

N.B. The Barometer's height is taken at one o'clock.

XXXV. *Letter to the Right Hon. the Countess of* Gosford, *on the Speculations of Theorists, particularly of the Neptunians. By* William Richardson, *D.D.**

In my former letter to your ladyship, I stated the arrangement of our materials in a very considerable portion of Ireland, beginning with its northern extremity, and stretching 130 miles along its eastern side, including a considerable breadth:—my object was to state facts in the reach of your ladyship and every one, from which it must appear that in the original arrangement of the materials of our world, the speculations of theorists, and more particularly of the Neptunians, received no support from the present order of things and the disposition of our strata as we now find them, upon the principles they lay down, and under the suppositions these wise philosophers make.

Hence they are forced to the necessity of assuming various changes to have place, of which we have no record, nor even tradition; and of introducing agents, for whose existence we have no authority, but the *ipse dixit* of these gentlemen.

Thus they, as well as other theorists, are used to get over all difficulties embarrassing their favourite theories, by calling in the aid of revolutions and convulsions, which, as they say, have deranged and disturbed every thing: but an attentive examination of any of our little systems, upon which I dwelled in my last, will soon show that *it*, at least, has never been *revolutionized*, and but very slightly disturbed; a faint, yet steady and uniform change, in the inclination of all the component strata, probably at first horizontal, now slightly inclined.

The local circumstances of each separate stratum, and its junction with those contiguous to it, without any interruption of the continuity or solidity of the heterogeneous materials, show plainly that they have not been acted on by any *violent* cause, since their consolidation.

From the local circumstances of our precipitous northern coast, I once showed in controversy, that these powerful instruments, *revolutions* and *convulsions*, so necessary to theorists, had never been in action in my country:—in reply, by way of compromise, it was offered to me by the Huttonians, that I should model my own country as I pleased, if I would not interfere with the rest of the world, but leave it to them to arrange.—I declined the terms.

Nor are the particular circumstances of our arrangements reconcileable to the general positions of the Neptunians, as I shall

be happy to show when they venture to state them; and that our strata, as we find them, could not have been formed by chemical precipitation, or mechanical deposition, from the waters of the chaotic fluid, as Mr. Kirwan supposes. This respectable countryman of ours, who hazards himself further than any other Neptunian I know, supposes the chaotic fluid to have been a mighty menstruum, from which our strata have been precipitated. That this menstruum, holding all the materials of the world in solution, must have been very powerful at first, but gradually abating in energy, as its materials were precipitated :—hence we should expect to find a perpetual gradation in the materials of our strata, the upper parts differing from the lower, in each separate stratum.

Nothing like nature; for we find the most decided uniformity take place in all parts of the same stratum.

Mr. Kirwan's theory is still more irreconcileable to the formation of a mass, or accumulation of strata, always passing into each other *per saltum*; and utterly incompatible with the formation of alternate strata, a most common arrangement.

Admitting the chaotic fluid to have been the grand matrix of our strata, and that agents we know not had altered the precipitating powers of the same fluid; the order of succession in the strata would have been still the same; and having the material of one stratum, we could tell that of the next it rested upon, and so on.

Thus mining, where the material we seek for is disposed in strata, (as coal,) would be the simplest science we have:—on the contrary, we have not ascertained any regular order in our strata in a vertical direction; and instead of pointing our efforts to accumulate facts from different and distant places, we recur to theory, assume modes of original formation, and thence deduce rules to which Nature shows she is not subjected, even that of *specific gravities*, giving us not the least assistance.

I know that those conversant with the subject, state certain indications, from which they infer the proximity of coal strata; but from our total ignorance of original formation, we have no clue to the order of arrangement, and cannot pronounce positively in any case, that we have coal strata beneath us.

There seems to be a broad belt in our northern hemisphere, through many parts of which coal strata are found at various depths; but the difficulty of determining where we may expect them is very great; I fear insurmountable, if we look for certainty.

Coal, ever since Marco Polo told us the Chinese burned black stones, appears to be an article of the greatest importance, and in some manufactures of prime necessity. Indeed the time seems

to be approaching when this valuable species of fuel will be con-
sidered as such in many parts of the north of Ireland: hence
great proprietors, and among the rest my friend the Earl of
Gosford, are anxiously inquiring into the probability of finding
coal on their estates.

We have mountebanks under m.·, forms, and I know none
more ignorant, or more likely to mislead, than those who claim
to be coal-finders.

I shall take the liberty of recommending to those proprietors
who are anxious to find coal on their estates, to consider the sub-
ject as a question of fact, not of theory and speculation; and
to act accordingly.

That is, to make the experiment at the least possible expense,
to find how *boring* can be executed in the cheapest manner;
and then to try in as many different places as may be found con-
venient, not under the direction of persons who claim to be
adepts; but of plain honest men, who, without speculating on
the subject, will report facts as they find them: these may
be transmitted for opinion to those most experienced in such
researches, and I know not any one who has taken so much
pains to make himself master of the subject, as my able friend
G. B. Greenough, Esq. late President of the Geological Society.

Some do not use such violent agents, but quietly elevate our
mountains and high islands, in the direction of an axis perpen-
dicular to the horizon; leaving to others to investigate the pro-
truding cause.

The Neptunians wrap their mantle-formed strata about small
elevations, until they accumulate them into mountains.

Whoever discusses questions with theorists, should be very cau-
tious as to the terms he permits them to adopt in stating their
opinions: they are fond of using the words *may* and *might;* they
should be told that in argument these should be changed into
did and *must.* The terms *possible* and *impossible* must also be
erased from the vocabulary of theorists; from the former no
conclusion can be drawn, and the latter is quite too self-sufficient
for such novices in the art of world-making to hazard.—They
should be limited to *facts;* and, in their deductions, subjected to
the rigid rules of logic.

I have been charged with being contentious and quarrelsome,
in having so often encountered these vain theorists; for, fully con-
vinced of my own ignorance of the early operations of nature, I
could not bear to see gentlemen as ignorant as myself, claim to
be adepts in the art of world-making, and therefore amused my-
self in exposing their fooleries.

My late friend, Earl Macartney, compared me to a person
at football, who, never daring to hazard *himself* by kicking the

ball, watched for an opportunity of tripping up those who ventured themselves boldly.

I have not always been so cautious; for although I gave up all hopes of explaining the earlier processes of nature, in the formation and arrangement of the materials of our globe, I have inquired carefully into the operations that have been performed upon it since its consolidation, and particularly into the formation of its present surface, so curiously diversified.

My sentiments on this subject have been held still more wild than the theories of those whom I have taken the liberty to laugh at.

I have sustained that the surface of our original world was once elevated above the tops of our highest mountains. That some mighty agent had acted upon our then surface, (probably uniform,) and had carried off its materials, irregularly and in immense quantities, reducing the globe to a smaller size with its present surface, without disturbing the materials left behind.

According to this position, the question, the formation of our mountains vanishes; they are no longer the stupendous monuments of powerful agents, or the results of grand operations of Nature, but merely the scattered remnants of a diminished world.

All this, your ladyship will say, is much wilder than any of the theories I have been ridiculing; but I must request you to distinguish: all these are mere matters of opinion; while my positions are given as matters of fact. The philosophers I allude to, claim to be acquainted with their agents, and show how they *might* have acted. I admit I know nothing of mine, but that he *did* act;—the results, that is the natural appearances, or facts, these gentlemen bring forward, or decline to notice, are at variance with, and irreconcileable to their respective theories. The facts to which I call attention, and in general the face of nature, whereever laid bare to us, afford as clear demonstration of the truth of my positions, as any theorem in Euclid is capable of receiving.

The question of original formation has been long abundantly discussed. Let us take new ground, and try to develope from existing facts the operations that have been performed on our globe since its final consolidation:— appearances which will justify the position, that our world did not come thus as it now stands from the hand of Nature;—that important changes must have taken place—that mighty agents, unknown to us only by the marks they have left behind them, must have acted upon it.

I shall slightly mention a few of the appearances that seem to countenance these wild suppositions.

1. The irregular diversification of our surface, quite unconnected with original formation, and the arrangement of our strata.

2. The

2. The isolated and highly elevated stratified hummocks, found in many countries, and especially in basaltic districts.

3. Whyn dykes, very lately brought into notice, but now bursting upon us in different parts of the world, in the form of walls of terrific altitude, and most regular construction.

4. Caverns, so abundant, especially on the confines of sea and land, and in calcareous districts, grand and beautiful grottoes, bearing irresistible marks of posterior excavation, not to be executed by any agents with which we are acquainted.

5. The line of demarcation between sea and land, and the formation of the basin of the ocean.

It has pleased Nature to lay bare the northern district of Antrim, and I may say its whole coast, to disclose her secrets to us, and to expose her arrangements to the naked eye; it is in our magnificent façades, so numerous, and so kindly extended along a great line of coast, that we are enabled to distinguish between *original formation* and *posterior* operations; and to pronounce upon the quiet state in which these original materials have remained since their final consolidation; notwithstanding the numerous revolutions and convulsions so confidently obtruded on us.

I shall in my next request your ladyship to accompany me to a new field, where the original arrangements of Nature seem to be the same with those in Antrim, and where posterior operations also bear a strong resemblance, and of course where my arguments drawn from facts receive further confirmation; I mean the island of St. Helena, upon which the attention of the world has been lately much fixed from causes unconnected with its natural history.

I am, with much respect,

Your ladyship's most obedient humble servant,

W. RICHARDSON, D.D.

Clonfecle, Moy,
February 25. 1816.

XXXVI. *On the Excitement of Voltaic Plates; in Reply to Mr. De Luc's Objections to the Doctrines maintained by the Author. By J. D. MAYCOCK, M.D.*

To Mr. Tilloch.

Barbadoes, May 23, 1816.

SIR,— I BEG to convey to Mr. De Luc my apology for not having noticed at an earlier period his observations on two papers, which Mr. Nicholson did me the favour to publish in his valuable Journal.* In July 1812, I left England for the West

* Phil. Journal, vol. xxix. and xxxi.

Indies,

Indies, and until the latter end of last March, not having had an opportunity of obtaining the subsequent numbers of his Journal, I was unconscious that what I had written had particularly interested Mr. De Luc.

I have carefully perused the papers which have been published by him subsequently to my quitting Europe, and have renewed my acquaintance with those which had been previously given to the world. In doing so, I have derived no small degree of pleasure, and much interesting and important information: but as I suppose the several points of excellence, with whatever defects they may be blended, will be sufficiently obvious to the scientific reader, I have confined the following remarks exclusively to those parts of his writings which have been urged against the doctrines contained in my papers; and I have endeavoured to observe such an order as will put the several points of difference between us in the strongest light. Had I remained in England, the particular train of thought into which I had casually fallen, would probably have led me to further speculations on the subject of electricity; but situate as I am, in an atmosphere extremely unfavourable for electrical experiments, without the means of obtaining philosophical instruments, except after the lapse of several months, from Europe, and with access to very few philosophical books, out of my own circumscribed collection; I have no new matter with which to enrich your pages, and it is with much diffidence I presume to occupy the time of your readers.

Mr. De Luc considers that he had published nearly twelve months previously to the appearance of my Essay on Chemical Affinity[*], a refutation of Sir Humphry Davy's hypothesis relative to the principles of chemical affinity, and the decompositions produced by Galvanism. It will be remembered that this hypothesis supposed that the elements of all compounds naturally possess different electrical *states* or *energies*, the one component particle being *positive*, the other *negative*; that this difference of electrical state is the immediate cause of chemical union; but that, when the compound is subjected to a Galvanic battery in a sufficient state of excitation, the particles possessing the *negative* energy are attracted by the *positively*, and repelled by the *negatively* electrified wire; and the particles possessing the *positive* energy are attracted by the *negatively* electrified, and repelled by the *positively* electrified wire; and that the force of these attractions and repulsions of the Galvanic wires to the component particles of a compound body, when greater than the attraction between the component particles arising from the difference of

[*] Phil. Journal, vol. xxix.

their

their electrical energies, is sufficient to subvert the combination *.

The experiments to which Mr. De Luc alludes, as particularly affording a refutation of this hypothesis, are the third and tenth, contained in the first part of his analysis of the Galvanic pile †, and to this paper I beg to refer your readers. Now, sir, I do not think that the result of these experiments invalidates in the slightest degree the hypothesis in question ; as a knowledge of the general principle in electricity, which has been termed *induction* or the *law of induction*, will enable us to reconcile the apparent disagreement. Upon this principle I will contend that the electrical states of the points 1. 2. 3. 4. were not such as Mr. De Luc supposes, but that 1. and 3. must, under every variety of the experiment, have been *positive;* 2. and 4. *negative.* I am aware that the application of this law to the particular case in question has been objected to. Mr. Singer considers it impossible for this law to operate, "unless the bodies be separated by a non-conductor, the resistance of which is sufficient to prevent the passage of electricity from one to the other ‡," and therefore, that it is altogether inapplicable to the phænomena of the " interrupted circuit;" the objection to its application being, that water is a conductor and incapable of affording such resistance. The experiments of Mr. De Luc §, however, very plainly demonstrate that there is such resistance in water as occasions a *retardation* and *residua* of electricity; in other words, such a resistance as prevents *a free passage* of electricity. That the situation of bodies being such as to enable electricity to pass from one to the other does not prevent the operation of this law, is fully established by the early experiments on the subject ‖ ; and I will contend that it is as fairly applicable to the interrupted circuit in Mr. De Luc's experiments, as to any phænomena of electricity: consequently I do not conceive that the reasonings of Mr. De Luc, founded on the result of his third and tenth experiments, are admissible; and I am inclined to consider the arguments contained in my Essay, the first valid objections opposed to Sir H. Davy's opinions on the subject of chemical affinity and decomposition ; and, in fact, the first if not the only refutation of them.

Having stated the results of my experiments with the Voltaic *plates,* I observed that they are subversive of the most commonly received opinion of the manner in which a Voltaic *pile* is excited, which originated in the phænomena of these plates, and " rests on the assumption that dissimilar metals while in con-

tact

tact are in different electrical states;"—in other words, that a difference of electrical state is produced by the contact of these plates. That the Voltaic plates indicate no degree of excitement whatever while in contact, but that excitement becomes evident after their separation, is not a *supposition*, but a *fact* demonstrated by the irresistible evidence of our senses; as much a demonstrated fact, as that all bodies of whatsoever nature fall through a given space *in vacuo* in the same time—the one altogether as unconnected with any particular hypothesis concerning the remote cause of electrical phænomena as the other is unconnected with any speculations relative to the remote cause of gravitation. But Mr. De Luc has promised to show the converse of my statement;—" to demonstrate by a great number of experiments, that these effects (*the excitement of the Voltaic* PLATES) exist only during the contact, and that it is owing to extraneous circumstances that any effect remains after their separation *."
I have naturally looked with much eagerness for this promised demonstration; but I have not been able to find one single experiment which militates in the slightest degree against my statement relative to the Voltaic *plates*, and which indeed, as it is a bare expression of facts, I do believe, will for ever remain incontrovertible. It however appears, that Mr. De Luc's promise to demonstrate the inaccuracy of my statement, has originated in his having unaccountably mistaken it; for he represents me as having asserted that excitement of the pile cannot be produced while the metals are in contact: whereas every one who has the slightest knowledge of the science of electricity, is aware that, in the more ordinary combinations for the pile, two metals have always been in contact; and that, in the kind of trough which was until lately used, two metals are soldered together. I am sorry that my observations should have been so misunderstood; as it has occasioned Mr. De Luc much pains to refute an opinion, which I could never have advanced in open defiance to common experience, and has induced him to suppose that I am unacquainted, not only with his experiments, and the very ordinary phænomena of the pile, but with the earliest and simplest facts relative to the science on which I had written. In truth, sir, his experiments with the *pile* have no reference whatever to mine with the *plates*. Let it be granted, that excitement took place in every dissection of the pile, in the manner and degree he describes, I yet affirm that there is no analogy between the *circumstances* necessary for the excitement of the Voltaic *plates*, and those necessary for the excitement of the *pile;* and that the hypothesis which attempts to explain the

* Phil. Journal, vol. xxxii.

excitement

excitement of the pile, by supposing that different metals acquire a difference of electrical state by contact, is not only unsupported, but actually subverted by the phænomena of the Voltaic PLATES.

In another instance, Mr. De Luc misapprehends in a most extraordinary manner the tenor of my observations. " I filled," said I, " one of the new porcelain troughs with an acid fluid, so that the metallic plates and their connecting arcs were completely covered. In this state a trough of ten pairs of plates three inches square decomposed water rapidly"—" I placed the metals connected by the bar in a trough without partitions, and no action ensued*." The result of this experiment was to me anomalous and unexpected. But Mr. De Luc has endeavoured to explain it, by taking *as data*, that " when the plates were immersed up to the bar in liquid," " the effect was reduced to that of one single pair†;" or, as I suppose, no perceptible action ensued. By a reference however to the experiment, he will find that such was the state of the apparatus when it decomposed water rapidly. He has therefore misunderstood the statement of the fact, but in no degree explained it‡.

I indeed regret that Mr. De Luc has misunderstood any thing that I have written; but I am altogether distressed at his questioning the accuracy of my experiments with the Voltaic plates; not indeed in consequence of having founded, as he supposes, a " *Galvanic system*" on them, which I am not aware of having attempted, but because I value, above all other qualifications of an author, impartiality and fidelity in narration. Mr. De Luc thinks some *extraneous cause* must have operated in *my* experiments, otherwise one single contact and separation of my plates could never have produced a sensible divergence of the gold-leaves : but he does not hint at any extraneous cause in particular, which he conceived likely to be the one ;—and as the apparatus employed and the manner of performing the experiments were minutely described at the time the results were given, any extraneous cause attached either to the one or the other might easily have been detected and exposed. Mr. De Luc states that in M. Haüy's experiments it required ten contacts and separations of plates the size of mine before the gold-leaves were *sensibly* affected. In the repetition which he made of these experiments with smaller plates, it required twenty repetitions before the effect on the gold-leaves was *visible*. Now, sir, I have expressed in my Essay, and so I wish to be understood, that a single contact and separation did not produce a *sensible*

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* Phil. Journal, vol. xxxi.   † Ibid. vol. xxxiii.

‡ I should like to know whether the result of the experiment, as stated above, has been obtained by other experimenters.

but

but a very *considerable* divergence of the gold-leaves. What must be the impression on the mind of your readers from this discordance of statement? Either that Mr. De Luc's and M. Haüy's experiments were very coarsely performed, or that I have grossly exaggerated the result of mine. Of the accuracy of my own experiments I cannot doubt, and it is in the power of any gentleman at a trifling expense to repeat them, and decide for himself. But, sir, you must be aware that I am not the only person who has obtained such results. I can at this time particularly refer to M. Volta for support. A description of his apparatus, manner of experimenting, and the result of his experiments, are contained in the following quotation : " Ces plaques ont trois pouces de diamètre: les metaux doivent être très-polis, bien dépouillés d'humidité, et appliqués l'un sur l'autre de manière à manifester une cohésion sensible. L'un d'eux doit être isolé, et l'autre doit communiquer avec le sol. On doit les séparer d'un seul trait et perpendiculairement: on fait toucher ainsi celui qui a été détaché au chapeau d'un électromètre, et on doit quelquefois un écartement des fils*." Volta's plates were three inches in diameter, mine five ; and as the areas of circles to each other are as the squares of their diameters, it will be seen that my plates were nearly three times as large as M. Volta's: hence one sufficient reason why the results in my experiments were so much more evident than in his;—another reason is however to be found in the extreme delicacy of my electrometer †. But how comes it that in Mr. De Luc's experiments it required *twenty* contacts and separations to produce the effect obtained by M. Volta by *one* contact and separation, although the plates of these philosophers seem to have been very nearly of the same size ? But, what is more extraordinary, how comes it that *ten* contacts and separations of M. Haüy's plates, nearly three times as large as M. Volta's, were required to pro-

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* *Hist. du Galvanisme par Suc,* tom. i. p. 254.

† This electrometer consists of a glass tube about 4½ inches long, its internal diameter a little more than an inch, capped with metal at each extremity. By one cap it is fixed to its pedestal; in the other cap there is a hole through which a small glass tube, an inch and half long, passes, and is fastened so that one half is within the body of the electrometer, the other without. The external and internal surface of this tube is coated with insulating varnish, and through the tube passes a wire, to one end of which may be adapted plates of any size: to one other is fixed a small pair of forceps which receive the gold-leaves. These are 1½ inch long and very narrow; their extremities are a full inch from the pedestal, and a little more than half an inch from the tinfoil.—Now the circumstance which, independent of the size of the instrument, contributes to make it more delicate than electrometers of this kind generally are, is the gold-leaves being connected with nothing but the wire—not with the cap, as is generally the case.

duce an effect equal to *one* contact and separation of M. Volta's plates, and an effect very far inferior to *one* contact and separation of my plates, to which they exactly correspond in size? Some *extraneous cause* must, indeed, have operated in the experiments of M. Haüy and of Mr. De Luc. Probably their electrometers were not very delicate; possibly they did not pay due regard to those circumstances on which depends the full action of the plates.

In reference to the production of Galvanism, I also observed that the Galvanic apparatus can only be excited by a decomposable fluid. This I meant to be equally applicable to the pile and the trough:—it is a fact which has been generally believed, but which Mr. De Luc thinks he has demonstrated to be false. With due deference to him, the following appear to be conclusions fairly deducible from his analysis of the pile, in reference to this subject.

1. The necessary combination for the greatest excitement, consists of two dissimilar metals with a moist substance *interposed between* them.

2. If this condition be observed, although *no two metallic surfaces be in contact*, but merely connected by conducting points, as in the first dissection, the effects will be the same, or nearly the same, as if the pile had not been dissected, but had remained continuous, in which state the different metallic surfaces are in contact.

3. If the pile be so dissected that two metallic surfaces be in contact, but the moist substance in contact with only one of them, if that one be the most oxidable metal, the powers of the pile will only be *diminished;* but if the metal in contact with the moist substance be the least oxidable, then will the power of the pile *entirely cease.*

4. A solution of *sea salt* is capable of exciting more powerfully than *common water.* The same pile which when excited by water was only capable of affecting the electrometer and producing decomposition, when excited by a solution of muriate of soda affected the electrometer, occasioned decomposition, and afforded a shock.

5. The power of vegetable and animal substances to produce excitement is, *cæteris paribus,* in proportion to their *moisture;* a pile excited by new cloth, which had stood some time in a room in which the hygrometer was at 40°, being only capable of affecting the electrometer, while the same pile excited by wet cloth affected the electrometer and produced decomposition*.

Mr. De Luc, in spite of these natural inferences from his own

* See Analysis of the Galvanic Pile.

experiments,

experiments, and without giving the grounds for his opinion, contends that the action of the pile does not depend on a separation of the binary groups of metals by a liquid or wet body, but requires only that the separation be produced " by the best non-metallic conducting substance." He thinks that in writing-paper, he has found such a substance; but let me remind him that all paper attracts moisture very powerfully, that it is extremely difficult to deprive it entirely of moisture, and that, when it has been perfectly dried, it very soon reacquires moisture. Thus in an experiment (mentioned by Mr. De Luc) in which the paper had been completely dried, the pile did not even affect the electrometer, until during the time occupied in carrying it from one room to another it acquired some degree of moisture: it then affected the electrometer in a very slight degree. In fact, I believe it might be stated, that whatever conducting power paper may possess, is dependent on the moisture it contains. Thus in every instance of excitement, moisture (*the decomposable fluid*) has been present: and from this view of the subject, I am somewhat inclined to think that the spontaneous electric column might be employed for hygroscopic purposes.

I would wish, sir, your philosophical readers to ascertain the legitimacy of the inferences I have taken the liberty to draw from the experiments contained in Mr. De Luc's analysis of the Galvanic pile, and to declare whether there are any facts deducible from those experiments, which tend to show a necessity for two metallic surfaces to be in contact to produce excitement; whether the facts do not establish a contrary opinion; and whether the results of all his experiments do not tend to prove, that "the Galvanic apparatus can only be excited by a decomposable fluid." Nevertheless, Mr. De Luc states the elementary principles of the pile to be as follows:

1. " In each binary group the *zinc* plate takes some electric fluid from its associate the *copper*."

2. " In each group also the *zinc* plate communicates through the paper some of its excess of the electric fluid to *the copper* of the next group on its side."

The first position is in direct opposition to my experiments; the second, M. De Luc draws from his thirty-fourth experiment: yet, as far as I can understand it, that experiment simply proves, that, the pile being excited, an insulated conductor will convey the charge of either extremity to the electrometer.

[To be continued.]

XXXVII. *On the Physiology of Vegetables.* By Mrs. Agnes
Ibbetson[*].

To Mr. Tilloch.

Sir, — The strange *inconsistencies* that subsist in the few facts
that have been acknowledged to be true in the physiology of
plants, must show that they are in many respects false, since
they contradict each other. Thus, plants are supposed to per-
spire, and also to give out oxygen. It is well known that the
water they take in decomposed, and reduced into its compo-
nent parts for the purpose. How then can they give it out in
oxygen and water also, when that water has been changed to air?
But they also admit that the cuticle takes in nutriment, and *all
from the same skin.* This is really giving the cuticle more offices
than Nature can perform, especially as they are all contradic-
tory. Then it is supposed that the blood circulates,—though
the juice is evidently expended, as it rises, in forming the parts it
is ordained to complete. If it does circulate, it must do so in
absolute *opposition* to the whole order of Nature; since even
no *animal respires* by a particular organ, except those that have
a real circulation: for even in animals or insects there is no
circulation where there is not a single heart, to which the blood
constantly returns, as the vessels that contain the liquid are so
disposed, that it cannot arrive at the other parts till it has
passed through the lungs. This of course cannot take place in
vegetables, which have no heart, nor in animals that have several
hearts.—See the admirable Cuvier.—Again, in the herbaceous
plants they may be generally opened close to the root, and the
flowers discovered aggregating there:—How then can the flower
but be formed at the top of the plant, and come out in a few
days also, without any preceding bud ? And in trees and shrubs,
if the flower-buds are all cut off when first appearing, a quantity
will very soon succeed ;—cut these away also, and *another set*
will reappear; this may be done *two* or *three times.* Is it
possible that all these flowers can proceed, or *be formed,* in the
few buds that were found at the exterior in *that place ?* No: they
are merely the vehicle through which the flowers in trees are
introduced to the exterior; and the first flowers are there de-
tained in the bud till the weather is sufficiently favourable to per-
mit them to pass outwardly. This is only learnt by dissecting
progressively, and pursuing the various facts throughout all their
appearances.

There is certainly a great difference between the tree and

[*] Erratum.—In Mrs. Ibbetson's paper in our last number, in the title,
for the word "*substitute*" read "*elucidate.*"

herbaceous

herbaceous plant, in the progress of the flower-bud. They both form it in the root; but in trees and shrubs they make several stages,—in herbaceous plants but two.

The first of the propositions selected for this letter, and which I engaged it should in a manner prove, *was*, " that the flower-bud was formed in the root, and of course that the root was the laboratory of plants;" since I before showed that the seeds were also *protruded there*. There are so many curious dissections appropriated to this subject, that it is *peculiarly by specimens* I must prove the truth of the fact. I shall first show therefore, by a series of prints (taken in progressive order from the plants), how the flower-bud passes in trees and shrubs, from the moment of its formation to its decay; that the reader may be as capable of judging as if he had the plants before him, in regular succession; since the specimens are the exact *prototypes* of the interior of the vegetable from which they are taken. And though every single specimen will not be given, on account of their number, yet I shall show the principal points of most of them, as they succeed each other.

A soft ball appears in the middle of the root traced regularly from its first formation, by opening a fresh plant every third day. This ball is then in a little time seen to pass up the line of life (a circle of cylinders discovered between the pith and wood), and which only enlarges at the flowering season for this purpose. This is never observed till a month or six weeks before or after flowering time. By degrees each specimen shows the ball moving up towards that aperture from which it is to break; while the cylinders naturally swell, and increase in size, according to the quantity of buds ready to pass up (Plate 1. fig. 1.) When they reach the part of the line of life from whence they are to be ejected from their place of concealment, the cylinder then opens, and one or two buds protrude, just opposite the ready made scales in the bark, which is in future prepared to receive them, when they shall have passed the *lignum* part. It is now they begin their passage through the wood, drawing a long string behind them (see fig. 2.). This may be seen, and disposed in different specimens in so many various ways, that a person must be blind or incredulous in *the extreme*, not to be convinced. In the first specimen the bud may be caught moving on, and drawing a string behind *through the beginning layers of wood*; then by cutting fresh specimens *horizontally*, and keeping them on the table a few hours, the bud, if then placed *under the microscope*, will pass (while under your eye) out of the wood. In a specimen of new wood, if the bark is taken off *gently*, all the buds (being still incased in the wood) are left there, and the scales alone remain in the bark; and in fig. 3,

the

the division between the bud and the wood, occupied by the
juice, is plainly seen preparing its way: and every person on
viewing this figure, and who is apt to notice what they see, will
recollect this perpetual mark in the wood (*o*), which is nothing
more than the passing flower-bud : but the bud (as I have be-
fore shown) does not force its passage, for the juice precedes it,
and has the power of bending the wood twigs both ways from
the bud, forming a *complete covered way* for it, that with the
help of the liquid it may pass without pressure : but as soon as
the bud has passed, the wood part recovers its usual situation,
and is restored by the help of the returning action of the mus-
cles, to *its usual position*.   There is scarcely indeed a more cu-
rious process in the formation of plants *than this :* " that so soft
a body should be able to pass (*without injury*) through so hard
a substance, is most wonderful. But does not the basket-maker
wet his twigs before he attempts to bend them? and do not the
sticks (if a wet sponge is placed between them) soon by a curve
leave each side of the sponge hollow, as the twigs have done at
fig. 3? and does not the juice before mentioned act for the same
purpose?   Thus all is *in nature*.   Before the bud reaches the
end of the wood (which it is some time in doing) the cradle or
winter bud (which is still empty) becomes covered with a thick
and *glutinous liquid, varnishing* her scales, to defend the interior
(or that which will soon become so) from the cold.   This I have
generally found to be the signal " that the embryo of the flower-
bud has entered her case."   Several specimens must be taken
about this time, each day, that the flower-bud may be well as-
certained to have entered her new habitation: her string will
still attend her, for she loses all power of continuing her journey
if. it breaks.   Much pains appears to be taken to prevent the
bud being hurt at this time; for though it is much covered with
scales, it must still be more exposed than it can be in the interior
of the plant.   I have repeatedly cut specimens perpendicularly,
when the bud was preparing to pass within its new habitation
and to arrange itself *there* ; and no part of the process can be
plainer : in some plants a part of the wood *accompanies the bud*,
and does not permit the bark to approach it:—this is the case in
the *marvel of Peru*.   No alteration is discovered in the next
few dissections: the flower-bud remains for a short time per-
fectly torpid in outward appearance, though the interior of the
bud is preparing for its next change.   Then is formed that cu-
rious specimen exhibited in my last letter, and in this at fig. 4.
This part must be pursued with great exactness, taking up a
fresh plant every day for dissection.   It will then be observed
that the bud is again moved, and thrown up into the new shoot. I
should never have discovered this, had I not observed the buds

throw

throw off their scales as they passed up under the screw (fig. 4.) which divides the new from the old wood; any one may see the scales fall off, and again renewed as soon as the bud has passed up into its place in the new shoot. Here is probably the reason why the new wood of every tree is always grooved, whether the old wood is so or not. The peduncle, in growing up the bud, generally gets incorporated with the stem: but if the bud is torn down some time after, it again stops at the same place from which it proceeded, by tearing down to that place, or at least in an exact line to that part of the screw under which it dipped: the flower has then only to protrude a common flower-stalk, as soon as it has quitted the bud, and to open; and this ends its whole history, at least that which belongs to the flower-bud of the tree and shrub.

I now turn to the bud of the annual and herbaceous plants, or those which rise each year from the earth. The difference is essential; since, instead of having a settled bud apparently visible for months, and as a promise of future flowers, the bud does not appear in general till just as it is going to break into flower, and till it has travelled to the top of the plant. Can it be supposed then that Nature, which in the tree makes the perfecting the flower a long process, should in the herbaceous plant complete it directly, without preface and without preparation, though it has literally the *same process to fulfil?* Does this appear natural, or even probable? thus to form the whole, and bring it forth without any time to mature its buds and juices? Impossible! Is it not strange that the curiosity of botanists should not have tempted them to tear open a plant from one end to the other, to seek the time when these flowers are formed, when they have already discovered the flower in the *bulb,* and that in the water-lily it leaves the root when quite large, and is to be seen with the naked eye? And in the *saxifraga crassifolia* it is quite as visible as in the crocus; and in the violet the flower-bud comes out of the root so much finished, that no person can doubt what it is, *if they will but look.* Its manner of mounting in yearly plants, yields specimens of *uncommon beauty.* *Unlike the tree,* the whole texture of the plant is *infinitely looser,* yet still consisting of innumerable cylinders one within the other; instead of being stretched tight, as at fig. 11, forming one only: the thin matter doubles in, and produces innumerable apertures at fig. 19, these folds afford a place of refuge for many flowers, indeed for whole bouquets: and if an herbaceous plant is cut horizontally with a very sharp razor, and then laid on the table for a few hours, the flowers will stand up *above the cylinders,* and thus exactly *distinguish the difference* between the cases and the buds, which from the extreme thinness and delicacy of

the

the matter, is not *easy to do*; for the cylinders are so folded in double or treble folds that to an eye unused to the sight, they would *all appear like the most beautiful* papier maché *flowers*, only *extremely small* and delicate, and perfectly *without colour*. But I have found another means of distinguishing the case from the *real buds*, it is the dark or *black shade* always attending the cylinders alone, as they are seen in contrast to the apertures, which being then empty, most plainly appear: but when the flowers mount them, they fill up the holes so thoroughly as to leave no *dark shade* to set them off. I shall give a specimen of cylinders without flowers (see fig. 6); and flowers without cylinders, at fig. 7; also fig. 13, joining both together, and *showing* (*though very badly*) the apertures up which the flowers mount. When you cut perpendicularly, and happen to remove the soft covering sufficiently to leave the flowers open to view, then they show *admirably*; for they are generally torn enough to remove the thin matter, leaving the various sorts of vessels discoverable between the cylinders. Of these vessels I can say nothing, but that I have most exactly copied them; but what they are for, except to support the flowers, I cannot tell; but we must truly imitate till we can *understand*. These large bouquets of flowers adorned with hanging spirals, and adding grace and beauty to the picture, I have truly delineated. But I must observe that they are *too small* to show all the *defects the tearing* might produce, though sometimes observable; and only exhibit the beauties arising from pressure, &c. which the *opening* and *closing flowers produce*. The most perfect pattern-drawer would be *left far behind* in this case: for so excessive *is the variety*, and so astonishing the beauty of these bouquets, that I in vain endeavour to *do them justice*. When some corollas half-open, they appear like finished flowers, see fig. 8, 9, 10. The *œnanthe*, the *angelica*, and the *hieracium spondylium*, will give an idea of some of these *pictures:* but to complete it I must show the different *specimens* from which each part is taken in the plants, as fig. 8*, fig. 9*, and fig. 10*, and the important of each specimen, just cut at the top of the root, that the reader may be a proper judge of the *curious manner* in which the flowers are arranged even in that situation. The root must be cut slanting. It must be remembered, that when the first specimen is observed, and when the flower-buds (in trees) *leave the root*, they run directly into the cylinders of the line of life. This is the same in the plants that rise each year from the earth: they also run into the same vessels, and are formed and conducted so far in the same manner;—but here ends the resemblance.

I have shown how different the formation of the line of life is, and how much looser all the matter as given in fig. 11*, the

variety of cuticles being divided into layers, and each showing
such a number of *apertures*, through which the *flowers shoot*
(see AA), instead of a plain and regular set of *sheets* (OO) *as in
trees*. But there is often, as I before observed, *a thing* which
seems to confine this flexible *matter*, and give the part the ap-
pearance of different shaped vessels, as at fig. 8, 9, and 10.
This I cannot well understand; but I have given each exactly
as it *appeared to me ;* subjecting both copy and original to the
opinion of many, who confirmed and reassured me as to the truth
of the likeness. However, when the flower rises above the root,
in herbaceous plants, each separate division shows again a more
marked line of life ; and if then the round stem is *cut* a little
slanting, this vessel will appear again with its bouquets, as it has
passed from the root upwards, (see fig. 12). After remaining a
time in this *situation*, a large collection of flowers begins to
gather in the middle of the stem, and you gradually see those
of the base of life begin to empty : it generally takes five or six
increasing specimens to complete the whole process; it is then
discovered that most of the flowers have left the vessels of the
line of life to gain this repository, (*b b*). But it is not one only,
but two or *three*, according to the size of the plant. Here they
receive their seeds and pollen, and visibly increase in size, and
then rise into perfect flowers by the growth of the stem (*ccc.*)
Their passage from *a a a* to *b b b* is most plain, and easily traced,
for the first hour after you have cut the plant; but it requires
the eye to be accustomed to the microscope, to see it well ; and
the moment the seeds enter the seed-vessels, they prove them
the flowers.

Can I then by any labour, or in any way, trace this series of
facts in a more *convincing manner ?* These specimens were all
taken from plants of the same kind in a series, as they appeared
a few days older than the preceding ; and the progressive mo-
tion from bud to flower, from flower to fruit, is not plainer with-
out, than it is in *the interior*. The difference between the tree
and herbaceous plant is just sufficient to account for the *winter
bud* in the former. It will be remembered that this is the fourth
year I have taken up plants in this manner :—The first year I cut
eighty-six trees; the second, seventy-eight; and so on : and such
loads of herbaceous, beginning long before they appeared above
ground, that I have learnt all *their winter process*. I am sure
I have cut many thousands within the last two years, subjecting
every part to the microscope. Does there appear then any
room in this picture for self-imposition or mistake ? I well know
how false reasoning is ; but I reason not, I only trace a series of
pictures presented in regular gradation by Nature; and though
exhibiting so many different parts, which it would seem almost
                                                    impossible

impossible to conciliate; yet, when joined together, they appear so consistent as not once to contradict or confuse each other:—the God of Nature alone could do this. That I have in vain for the last two years gone over and retraced every part:—the more I dissect, the more absolute my belief becomes. And where are the facts strong enough to make us sufficiently incredulous to deny our eyesight? Nowhere, but in a few *mutilated propositions*, most of them showing their *falsehood* by contradicting each other.

It is impossible to give to the public a complete series of facts, copied from Nature, and which forms a quarto in itself, in a more disadvantageous manner than the present, since the first letter is forgotten before the second is read, especially as these facts (like a mathematical problem) all hang on each other, and depend much on their *general consistency* for the proof of their truth:—but I have already sacrificed sixteen years, and to hazard fortune on the publication also, is too much. Before I close my proposition respecting the flower-bud, I' must add one proof, which I have shown to many. When a quantity of young buds have just appeared above the stalk (suppose in umbelliferous flowers), if the first set are cut off, the aperture in which the flowers are rising up, is often so hollow and clear, that, look down near half a line, and you see the other flowers and buds coming up to supply the place of the dilapidated ones, or those which have died away. Sure this also cannot be vain; it must prove the flower is formed below, or all Nature is a deception! But we prefer gaining by reasoning, rather than by our eyes; the latter often much *more just*, and more to be trusted to. I must add, that experiments made on the *living tree* (if it is to stop any *vessels*, or *alter* the course of nature) are *not to be trusted*. I experienced this in cutting off half a bean, and replacing it in the earth: the root, instead of coming out at the top, at the same orifice as the stem (as it always does), took a shorter road, and left the bean at the place it was cut. In this manner Nature will deceive us—if the proper passage is stopped it will form a new one for itself. But *watch her*, and she will ever be found the same: and if I only lay open a plant (without attempting to stop *its vessels*), it will, though languidly, continue its motions for near an hour, because its functions are all lengthways; and I shall not have impeded any of its actions, but only displayed them. The flowers therefore will continue to rise for a short time, owing to the motion of the muscles.

I now turn to my second proposition; "That the leaves are the lungs of the plant." This is universally allowed; yet I never heard a *reason given* "*why they are so.*" For the leaves

forming

forming oxygen by the decomposition of water, is no *reason at all* for such an appellation; and it is certain that the leaves contain vastly less air in their interior, than any other part of the plant, and *are not* therefore a *vehicle for air*. But though they have no air within, they still merit the name they have acquired, by being the constant cause of the motion of air *at the exterior;* so that the very oxygen the leaves give out would probably remain *almost stationary*, on account of its weight, under the trees, rather than circulate around, were it not for the innumerable little fans that by their incessant motion produce an excessive circulation, which is rarely stopped, and is most violent in the *lowest spots*, and where the general stagnation of air is most likely *to exist*. But the motion of the leaves not only changes the *gases* (which descend from the higher regions by the help of the currents of air), but increases also the *natural evaporation* of the leaves. How exquisitely beautiful is then the arrangement of the lungs of the plant, when they are considered as set in motion by the spiral wire or muscles of the plant, in order to disperse the oxygen! and *that motion* exactly proportioned by Nature to the situation of the ground, and the necessities of the sort of country in which they are placed! On the high hills of Scotland or Sweden, where no putrid air is discoverable, the *firs* grow, which give but little oxygen; for they have no swamps to rectify, no animal breathing to purify: the natural motion of the air is therefore exercise enough for them, and to disperse their pollen. Hence the firs have no spiral wire in their leaves, and in *their leaves no motion*, and fewer muscles (except in their wood) than any other plant. But behold the contrast:—The *low* and *swampy grounds* loaded with aquatic plants and trees, where constant motion is necessary to the purifying of the air—Here Nature not only bestows a quantity of oxygen (which its trees emit continually), but she has loaded the peduncle of the leaf with a quantity of spiral wire, which keeps its leaves in perpetual motion. View only the *abele*, or the black poplar. It is not because its *leaf-stalk* is *broad* one way and *thin* the other (see fig. 14), that the leaf is for ever moving: this shape would cause its constant action, when the *wide part* of the leaf was *facing the wind*. But what (but the muscle) could keep it *constantly in that position?* The *muscles alone* could do this, by contracting and dilating it, *according to the dryness* or *moisture* of the wind *that blows*. It is by this means the leaf-stems of all the poplars are a trifle more or less turned to the wind, and in this position they will ever be found. Those plants have most spiral wire which grow in swampy grounds; those trees have most motion that are buried in low valleys, where

both

both air and water at times would almost become stagnate if it were not for the spiral wire in the leaves. But so beautifully has Nature contrived her laws, that the very moisture of the water, by causing constant motion in the leaves, gives also increased motion to the water, and that water additional freshness to the air; while the deep valleys, which have quantities of aquatic plants, want (more thoroughly than any other) to have the oxygen mixed with all other gases, to purify the atmosphere. How exquisite then is this continual interchange of benefits!—how delightful then the discovery, "that not only the quantity of oxygen is doubled in those vegetables found in low grounds, but the spiral wire is also increased in an equal degree!" Thus botanic physiology perfectly agrees with atmospheric chemistry, to *enforce the welfare* and establish the happiness of each animal that lives and breathes; and not only of those whose scent and wholesome breathing is thus *secured*; but of those inanimate beings who receive health and nutriment from the very spoiled air that is thus drawn off, and imbibed by them for their benefit. Hydrogen and fixed air being in small degrees serviceable to plants in general, though they will not grow in that air alone, their absorption is of the utmost use to man and animals. Thus, if the spiral was not the muscle of the plant, how could all this be brought about?—how could the leaves be moved in stagnate situations?—how could the oxygen get thoroughly mixed in the higher regions? It would remain under the trees, to do harm to the vegetable world; and so far from curing the bad vapours in low morasses, they would be left in their putrid state, to give death to those who entered the valley. As the moist winds act on the leaf of the poplars, so do they also on the corolla of flowers, turning the back of the *antirrhinum* and pea-flower to defend the pollen from the moist winds. How exquisitely sensible are the muscles of the plant! If a moist wind blows, they will lengthen more and more, till they have lost all the twist of their spiral wire:—if a dry north or east wind is felt, the muscle will *contract to half its length.* If there is no muscular contraction, what makes the *malva* flower in certain dry winds push off the whole of its corolla, by contracting the calyx to such a degree as absolutely to *pinch off* the petals altogether? I have often seen above twenty flowers thus forced off, one after the other, in an over-dry season. Those who do not look on the spiral wires as the muscle of the plant, but as sap-vessels, *should show us* how a flower is opened and shut; how the tendrils *twist*, both *within* and at the exterior of the flower; how those *tendrils twist,* that cover the whole *surface of a plant in the way of hairs;*—but particularly how the *wood warps.* They

M 3   proceed

proceed evidently all from one cause. What power acts on it?
What action remains so long after the death of the tree? We
do not believe in witchcraft, and *an action must have a cause.*
A being dies: all motion ceases after death, *except one,* which is
involuntary muscular motion. The wood warps after death, it
is full of spiral wire: it is *this* which most *evidently causes its
motion* since, if you take a part of it out, all motion ceases, and
that *part taken out moves continually.* If this is the case, it
must be the *muscle of the plant;* and the warping is at once ac-
counted for, being the only part, the animal which moves after
death: it is also the only part of the vegetable which *retains its
action after the vital power is extinct.* We are not to judge
a living being by the *laws of non-existing matter;* that matter
which is made and joined molecule to molecule, may increase
by *heat,* which divides *these parts* by *separating them at a
greater or smaller distance.* Thus *iron* is increased by the
quantity of caloric introduced between *each molecule.* But how
can this law be carried into a *living body?* Vitality is actuated
by a totally different power, and partakes completely of the
animal in this respect, increasing in continuity; and forced to
action by the power of the *muscles only,* after vitality should be
dissolved.

I think I have so exactly marked the difference (with the help of
Mirbel's ingenious idea) between the animal, vegetable, and mi-
neral, that they can no longer be considered as flowing into each
other, or making a series of steps, but perfectly disjointed, and
different from each other, and peculiar in all their parts. The
animal having *life, brain, nerves, muscles, voluntary* and *in-
voluntary motion.* The vegetable *life,* but neither *brain* nor
nerves, but irritability of muscle even superior to animal life[*],
these serving instead of nerves. Hence in death the *vegetable*
cannot be considered as a being to be contracted or dilated, as
iron or water, and which is removed by heat molecule *from
molecule;* but as a living creature, which has muscles to move,
and which, when dead, can only be subservient to the action of
the muscles for a time, and which are, like all vegetable muscles,
set in motion by the powerful change of light and moisture, but
subject to no other contracting or dilating power. And if in
such full proof we want an additional one, to show that the spiral

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[*] This irritable or contractile power, is that property by which mus-
cles recede from stimuli; it is independent of the nerves, and so little
connected with feeling, that upon cutting away all the *nerves* and stimulat-
ing the muscle with a sharp-pointed instrument, or a caustic, or directing
the electric spark through it, the muscles instantly contract, as does also the
vegetable.

is the muscle of the plant, we shall find it in the great discovery of HALLER, in stimulating the muscles of animals by *caustics* and sharp-pointed instruments: the spiral wire being equally affected *by both*, retiring (if quite fresh and in health) from the accession or touch of either.

By means therefore of the muscle of the plant throwing the leaf into action, the leaves are most properly denominated lungs to the plant. But this is not all the office of this part of the vegetable. In the leaf is mixed that juice of the bark by chemical affinity which contributes to altering the colour of the blood of the plant, so changing by means of the oxygen the dark resinous thick blood into a fresher liquid of a more florid colour, and thus reducing it also into a thinner juice, which enables it to run with speed down the inner bark vessels at the bottom of the leaf, which lead directly down to the bark. The first part of this is exactly what our lungs do; and this alone would enable the resinous juice to flow with ease to the bark, when first made in the leaves of the vegetable.

I shall now turn to the third and last proposition I am to give in this letter, which is equally new, and taken from the dissection of plants. It is "that the corolla of a *flower* is formed by bubbles of water placed in rows, and owes all the beauty and lightness of its tints to the refraction and reflection of the sun on the balls of water which compose its pabulum."

The corolla, to be known, must be taken to pieces. There is some art required to do this; for, if the petals are at all pressed, they are destroyed; they soon break their bubbles and spill their liquid, and thus spoil the whole specimen (as may be seen by pressing one). But it is possible to take the petal of each different corolla, and, splitting it, draw off the upper and under cuticle, and, leaving only the middle part to be examined, "that is the pabulum," to gain the most exact result;—since the different cuticles will then (if placed in the microscope) properly so arrange themselves (according to their size and focal distance), that, though there are several separate layers, yet they are so little divided, each will rise to its proper height, and *enable* the eye to distinguish them from each other, and not in any manner confuse their parts together. Taking the corolla in this manner, the pabulum is soon discovered to be balls of water laid in rows; and this even the naked eye in some flowers will show; and these bubbles of water (covered only by an extremely thin skin) lined by an impervious one, so clear as often scarcely *to appear* to the naked eye.

The petals of most flowers differ from leaves in many respects, but particularly in one essential point:—in leaves, the coloured

M 4   skin

skin is within, the *white* is *without.* But in flowers, the pabulum is *white*, and the upper and under cuticles *coloured.* To the pabulum the petals of flowers are indebted for their brilliant appearance, and not to the juice which inflates them (which is generally of a dull and livid colour); but the bubbles receiving the rays of the sun, and returning them to the retina through these colours, paint them with a *vivid glow* impossible to express in words; but easily shown by throwing the light of the sun through a small glass bubble on the dullest colour imaginable, and it immediately returns the brightest of tints.   Thus these bubbles receiving the rays of the sun (which strike each drop of water) are enlivened and enlightened by the reflection and refraction of that bright ray of light seen in every bubble, and striking the retina, by which means the whole flower would become *a blaze of light* too violent for the eyes, had not Nature, to soften it, covered it with a cuticle of a gauze-like texture, which, refracting each ray, gives it a *softness and beauty* seen only in flowers.   This being their form, it must stand to reason, that, in spite of the upper cuticle, much heat must be evolved; and yet it did not occur to me to measure it, till I received a letter from Sir J. E. Smith, in answer to one I wrote to him, respecting the raising the thermometer during the fructification of the seeds; when he requested me to see whether the polished surfaces of the petals were not the cause of heat still more than the seeds.   This immediately set *me to work ;*—the only trial I have ever seen on the subject was one made by the excellent Mons. Hubert in the island of Bourbon; but it is given by Mons. St. Vincent in so strange a way, that I cannot make it out.   In the first place he says that the maximum of the heat was at sunrise.   " That Madame Hubert, who was blind, was much struck by finding the plant *feel hot* to her hand; and that when the thermometer was applied to the spadixes of the plant, it rose to 30° of Reaumur, the standard thermometer being 18°."   Now this in Fahrenheit would be 50° and 62°.   Now how could Madame's hand, which even supposing her *seventy*, could not be less than 75° in that climate, feel 62° *hot to her hand ;* when it was thirteen degrees cooler than her own flesh, and would therefore be cold to it ?

I cannot help thinking that there was so much handling, and cutting, and placing the plant round the instrument, that the hand must have communicated much of the heat it possessed to the vegetable it held.   It certainly was neither the corolla nor the seeds that gave it; since the male and female were cut to pieces and disposed round the thermometer, and all motion must soon have ceased in thus dismembering the plant.   However, we
                                                                        are

are not fair judges without trying the same plant the same way. When he tried the common *arum* in the interior of the corolla in the sun, it gave 6 or 7 above the state of the atmosphere. I never got Hubert's account till after my own trials were completed; but I have since tried to cut the spadixes of the *arum*, and place them in the manner directed; but it had no effect on the thermometer.

I shall now explain the trials I made both before and after I received Sir J. E. Smith's letter: every thing that could be done to guard the plant from receiving any heat from the hand was done. Having arranged the approaches round the flowers to be tried the evening preceding, a stick was placed to which the thermometer of comparison was affixed, and a contrivance which enabled me to slip on or off a paper cover, when I wished to try the seeds, that I might not, when the pericarpium was below the flower, be obliged to pass the instrument through the corolla, but into the seed-vessel at once, without the bulbs being exposed to the atmospheric air: some sticks were so placed that I could run the thermometer into the flower without injury, by the help of a pair of long pincers, and the whole was covered with a large umbrella to be turned off and on, as *required*. I began at seven o'clock in the morning.

	Therm. of Comp. In the Sun.	Therm. of Trial.		Therm. of Comp. In the Sun.	Therm. of Trial.
Iris	57½	63½	Arum	53	61
Arum	55½	64	Hyacinth	56	62½
Arum	55½	67½	Iris	59	68
Hyacinth	49	58	Rose	57	64
Rose	57	64½	Honeysuckle	55	63½
Honeysuckle	55	63½	Arum	59	66½

Seeds flower in Paper.			*Seeds flower in Paper Cover.*		
Arum	55	57½	Arum	55	56½
Rose	58	60½	Iris	59	61¼
Iris	57	59	Rose	63	65½
Honeysuckle	59	62½	Hyacinth	49	51¼
			Lily	61	63½
			White lily	60	63½

When I tried the seeds, the bulb was placed in the midst of them, without passing through the corolla. The result may, I think, therefore, be fairly stated; that the greatest part of the heat (which was supposed to result from the seeds only) certainly comes from the corolla: for, cover the thermometer when taken from the open air by any thing, and it will rise nearly one

degree;

degree; the seeds gain only 2¼; if indeed it is any thing, the
quantity of caloric must be *very small*, and only at the time of
fructification, which may arise from the quicker motion of that
season; since it is certain that at no moment the seed is liable
to so hasty a revolution, as when the inside of the heart first
forms itself. But the heat the corolla gives is a decided heat;
and it to be discovered in every *flower* that will admit the
bulb; and I should suppose intended to accelerate the fructifi-
cation of the seeds, the completion of *the juice* of the pistil, which
for the purpose above mentioned may require to be raised to a
certain temperature before it is fit to pass down the pistil into
the seeds. I always shaded those flowers that were afterwards
to be exposed to the sun, till two or three minutes before the
experiment began, or probably the heat would have been higher.
But it is not certainly owing to the *polished surfaces of the pe-
tals*, for nothing can be less smooth in the *microscope:* but it
is owing to the balls of water that compose the pabulum, which
reflects heat from each bubble of water and light; and if the sun
is hot enough to decompose the water (which it certainly does
in those petals that have hairs), it of course must reflect *great
heat* from the pabulum; and this may always be increased by
taking off the upper cuticle. But it is not to one sort of form
the corolla is confined, though, except the Everlasting, the *pabu-
lums* are all formed of bubbles of water. But there are wet pe-
tals, such as the hyacinth, which, though they are so filled with
liquid, it is quite wonderful how the skin can keep it from
oozing, yet detain the liquid in such a manner as not to wet the
hand on which it reposes. This is really a wonderful thing, and
shows in how great a perfection Nature has made these *skins*,
which not only may be so filled with liquor as to be greatly in-
flated, but also so thin as to appear perfectly transparent, yet
cover the water sufficiently to keep it from too great an evapo-
ration, and enough to allow it to decompose water, which the
corollas that have hairs only do.

There are still many kinds of corollas, one sort in particular,
which we have so essential imitated in forming velvet, that it is
impossible not to be struck with the similarity of the formation.
The upper coloured cuticle is formed of a vessel carried up and
down in scollops, and then cut at the top;—this appears to pro-
long the ray of light which falls on it, instead of absorbing or
repelling it suddenly. It is thus it acts on feathers, it is this which
gives them so exquisite a success. It is this also in miniature-
painting which makes a stroke so much softer than a dot. It
is this on the cheek of beauty when the soft down shades it,
which is so exquisite to behold, and which paint so wholly de-
stroys. The like petals, which are mostly flowers belonging
                                                          to

to a hot country, have probably their corolla formed of greater consistency, that the water may not too soon evaporate in the bubbles. When the corolla is thick or thin, it is the *increase* or *decrease* of their pabulum only to which that circumstance is owing,—every other part is *the same.*

The Everlastings have the pabulum made of powder instead of water, and certainly give *no heat* whatever, but retain their form and beauty for a long time, not fearing the general enemy of flowers evaporation,—dust being their only destroyer.

But the most curious plant in respect to dissection is the *ranunculus ficaria* and *bulbosus*, which have their pabulum covered with a brilliant white powder, which seems not only to refract great light and heat from the bubbles, but from the powder also. But such was the badness of the weather, that I have not been able to try the heat they give; the flower is indeed almost too small to introduce the bulb of the thermometer.

I shall now touch, though gently, on the means by which these flimsy bodies (the petals of flowers) are sustained and strengthened, so as to bear much pressure and much ill usage, and to preserve their elegant shapes in spite of wind and rain. No one would for a moment doubt that the spirals governed the petals of flowers, if they would watch them for a few minutes as I have done for days, nay weeks together.—How often have I seen one of the petals contract its motions before the rest were at all sensible of the impulsion; twist and furl its sail-like wings, lay them fold on fold, exactly according to the drawing of the spiral! I know not any better means to show the muscles, and how completely the spiral is that part of the vegetable, than exposing them to a variety of temperatures—both extremes of heat and cold will act equally on them; they contract, then lengthen, and at last become vapid and dead,—lose all the stiffness of their spiral, which untwists and breaks, and the water immediately evaporates from the bubbles:—you then plainly see the tube in which the water was confined: and I do not think I was more surprised than to find that each bubble was opened by a *spiral wire*; but they are not smaller than the eye of many insects, in which so much *mechanism* is discovered.—We are for ever to be reminded, that to Almighty power there is no small or great.

In the corolla Nature seems to have sported with a vivacity calculated to exhibit her powers: and beautiful is it to follow the pea or the bean in its various species, which discover a variety quite as astonishing; and when the simple direction of the muscles is not sufficient to manage its folds, a certain piece is formed like the strengthening piece of an instrument, being a treble fold of the pabulum, from which the vessels allowed to take

their

their exit as from a centre, and it serves as a strong part to accelerate and fortify the powers before given to it by the muscular force. I think, any one who will examine the strength of the bean banner on the back of the red *antirrhinum* with care, can never attribute such force to any thing but a muscle *pervading* all *vegetable life*. The foldings of the last-mentioned flower are artful and powerful, and the springs so admirably contrived as to merit peculiar attention;—most of the flowers of that kind have *two side-springs* to connect the two parts of the corolla together; they are unusually strong muscles. Try their strength, and they will be found powerful; they lock one within the other: but when the flower is dead, if the spring is examined, all the spiral will be found dead and unwound, and broken in various places.

I shall in a future letter give some beautiful specimens of the mechanism Nature contrives in the opening and closing of the flowers. The mechanic power is *easily seen*, so plain and simple as to explain itself. I shall also give a guess, " why Nature does so at such different hours." It is a curious subject: but a few general principles will be found to *disseminate their powers* in a such a manner throughout all *vegetable life*, as to be *fully competent* to manage *the whole ;* and I hope by continuing my dissections, and trusting to Nature only to prove her own work, she will bring conviction of this important truth, and that I have nothing to do in it, but *obey and follow her.*

<div align="center">

I am, sir,

Your obliged servant,
</div>

Dawlish, Sept. 1, 1816.   AGNES IBBETSON.

<div align="center">

Sketch of the Plate.
</div>

Fig. 1. A specimen of the cylinders, which always enlarge at top and spread when cut; the flowers rising in them, and arranging themselves in a bouquet, when no longer pressed by the vessels.

Fig. 2. Dissection of the wood, cut perpendicularly, showing the buds AAA running to the bark.

Fig. 3. The bud in the wood, showing it running through the wood, with the juice surrounding it so as to guard it from being touched by the hard part.

Fig. 4. and 4*. Both showing the manner In which the bud is thrown up into the new shoot, and the cause of every new shoot being grooved.

Fig. 5. Manner in which the vessels of the line of life in trees are often formed, marking a great contrast in those of the herbaceous, which have innumerable folds.

<div align="right">Fig,</div>

Fig. 6 and 7.—6 being the manner in which the platform appears, or apertures through which the flowers pass; and fig. 7. shows the buds without the platform: and fig. 6 and 7 both together at AA.

Fig. 8, 9, and 10, are the three specimens, showing the very curious manner the folds of the thin matter will draw in: and fig. 8*, 9*, and 10*, are specimens of the root of the *œnanthe*, the *angelica*, and the *hieracium spondylium*; out of which the foregoing figures were taken.

Fig. 11. and 11* are the specimens showing the manner the line of life is formed, when folded in trees either so or in fig. 5: and 11* the manner it is folded in herbaceous plants.

Fig. 12. Manner in which flowers mount in the stem of herbaceous plants: first in little bouquets, then collecting in large, as at *eee*, when the stem lengthens, and they open.

Fig. 13, showing the apertures through which the flowers mount.

Fig. 14, the stem of the leaf of the poplars.

Perhaps I had most wisely avoided giving the figures described at figs. 8, 9 and 10, as carrying so little probability in their appearance;—but when I first began to dissect and imitate the vegetable tribe, I most absolutely determined that I would literally draw all I saw, without exaggeration and without diminishing the objects presented to my sight, let them be ever so extraordinary. No one had before taken a review of these objects,—all that was supposed to be known, was very little more than conjecture, except the seeds: no one had attempted to take the specimens *progressively*—what the interior was, therefore, " *was* (till now) *a secret;*" and after sixteen years constant dissection I cannot be accused of ignorance. With this observation I leave it to public opinion.

XXXVIII. *On Sir* H. Davy's *Safe-lamp for Mines. By* John George Children, *Esq. F.R.S.*

To Mr. Tilloch.

Sir, — I read with some degree of indignation, in the Annals of Philosophy for July last, a paper by a Mr. Longmire, calling itself *Remarks,* &c. on Sir Humphry Davy's Safe-lamp for the Colliers; and I addressed a letter to Dr. Thomson on the subject. That letter the learned Editor of the above Journal has thought fit to suppress;—nor should I have considered it worth while to take even this brief notice of so weak an attempt to injure one of the most important inventions with which enlightened genius has ever blessed the world, but have left it to perish in its own

insig-

insignificance and be forgotten, had I not found the subject pur-
sued in a letter from a Mr. Holmes, in the succeeding number
of the same publication. The avowed intention of the author
of this paper, and the experiments on which he pretends to
ground his objections, seem to me to require some animadver-
sion, not indeed from the liberality of the former, nor the re-
liance (as I shall show presently) that should be placed on the fi-
delity of the latter, or the inferences deduced from them; but
from the *possibility* that, if they pass altogether unnoticed, an
opinion may prevail, among those who have not sufficiently con-
sidered the subject, that they are founded in truth.   As the re-
sults of Mr. Holmes's experiments are diametrically the reverse
of similar ones by Sir Humphry himself, I thought it right in
the first place to examine their accuracy; and with that view I
submitted the safe-lamp to the most rigorous trials, and under
circumstances as analogous to those which prevail in the coal-
mines as I could devise.   For this purpose, I caused a cylinder
of strong tin plate to be constructed fourteen inches in length
and five in diameter, having four tubes in the bottom half an
inch wide, and one inch long each, placed two and two opposite
one another and one inch from the circumference; and three
other pairs of similar size, fixed in the sides of the cylinder, each
tube on the same level being opposite to its fellow.   The lowest
pair were three inches from the bottom, the middle six, and
the upper pair nine inches distant from the same point.   By
means of these tubes I could expose the lamp to atmospheres of
various degrees of inflammability, and I could also force the gas
into the cylinder with greater or less violence, and at different
heights, from bladders connected with stop-cocks, as well as
project into it such substances, in fine powder, either above, be-
low, or on a level with the flame of the lamp, as might be thought
likely to communicate explosions, through the wire-gauze, to
the atmosphere surrounding it;—and I could distinctly observe
the phænomena through small squares of glass fixed in the sides,
so as to afford a full view of what passed in the interior of the
lanthorn.   Not having any of the real fire-damp of the mines,
I substituted for it the more inflammable gas obtained from the
distillation of coal, and consequently the trials of the safety of
the instrument became so much the more severe.   The lamp I
employed was made by Mr. Newman of Lisle street.   It is nine
inches high, and the gauze cylinder, which is constructed of
simple iron wire, is $1\frac{1}{4}$ inch in diameter—the diameter of the
wire being $\frac{1}{63}$d of an inch, and the apertures, of which there are
960 to the square inch, $\frac{1}{30}$th.   This lamp gives an excellent
light with spermaceti oil, which continues undiminished many
hours.—I will here briefly mention Mr. Holmes's experiments,
and

and then state those which I made. I should perhaps have rather given the *credit* of these experiments to Mr. Ryan, as it seems it was he who first made them; but as he " practised on a lamp of only one inch diameter," he coincided with Mr. Holmes in opinion that his trials were unfair; and Mr. Holmes having procured a lamp from Newman, the following experiments were made by himself and Mr. Ryan, partly at the gas-works in Dorset-street, and partly at a chemist's, in the presence of four other gentlemen.—I quote Mr. Holmes's words:

" I tried it first over a small gas tube, with coal-dust and powder, which ignited the gas outside; next with coal-dust alone, which after repeated trials produced the same results, and left an inflammation at the end of the tube."

" On inverting a teacup over the cylinder so as to *produce a slight compression* of the gas, it exploded from coal-dust several times."

" We then went to a chemist's and forced some gas from a bladder against one side of the cylinder, while gas from another bladder was gently pressed on the opposite side: in a short time the gas on the outside inflamed;—this I compare to a blower, although the power we were able to use was very inferior to what would be given by the velocity of a blower under ground."—" I found that the flame of the wick would not penetrate the gauze cylinder, but the inflammation of the gas would, when acted upon by a strong current of air."

I shall now state my own experiments:—I suspended the safe-lamp in the centre of the lanthorn, at about two inches from the bottom, the four bottom apertures and the top of the lanthorn being open—I then forced coal-gas, from a bladder, with all the violence I could into the lanthorn through one of the lowest side apertures, an assistant at the same time throwing in atmospheric air from another bladder, through the middle aperture, on the opposite side, the other lateral apertures being closed. In a few seconds, about two inches of the wire gauze became red hot, and by continuing the blast from each bladder rose almost to whiteness, the heat being greatest at the side opposite the jet of atmospheric air.—At this point the exterior gas exploded. In a former experiment of the same kind, in which the heat of the wire was not raised above a low red heat, no explosion ensued.

Mr. Holmes says he " cannot find that even this distinguished chemist" (Sir H. Davy) " has been able to explain why flame will not flow through small apertures."

Mr. Holmes cannot have taken much pains in his search; for in the first part of the Philosophical Transactions for the present year, at page 117, Sir Humphry says as follows:

" If a piece of wire-gauze sieve is held over the flame of a lamp

or

or of coal-gas, it prevents the flame from passing it, and the phænomenon is precisely similar to that exhibited by the wire-gauze cylinders; the air passing through is found very hot, for it will convert paper into charcoal; and it is an explosive mixture, for it will inflame if a lighted taper is presented to it, but *it is cooled below the explosive point by passing through wires even red hot*, and by being mixed with a considerable quantity of air comparatively cold. The real temperature of visible flame is perhaps as high as any we are acquainted with—Mr. Tennant was in the habit of showing an experiment, which demonstrates the intensity of its heat. He used to fuse a small filament of platinum in the flame of a common candle; and it is proved by many facts, that a stream of air may be made to render a metallic body white hot, yet not be itself luminous."—I forbear to quote more of this interesting paper, though I recommend the whole to the careful perusal both of Mr. Holmes and the other opposers of the "wonderful lamp."—The experiment I have detailed is correctly explained by the above reasoning. Even at a red heat the wire cooled the gas below the point of inflammation,—but not when the heat nearly reached to whiteness, and accordingly explosion then ensued. Mr. Holmes has not, after all, made any great discovery in *finding* that the inflammation of the gas will penetrate the wire-gauze when reacted on by a strong current of air—for Sir Humphry Davy has stated it himself in his "Additional Practical Observations," printed first for distribution amongst the miners, and which may also be found in the Philosophical Magazine for July last. I quote a few lines from that paper on the subject: "When indeed a *strong current of coal-gas is driven from a blowpipe* so as to make wire-gauze of 676 apertures strongly red hot in the atmosphere, the flame from this pipe may be passed through it whilst it is strongly red hot; but this is owing to the power which wires strongly ignited possess of inflaming coal-gas, and they have no such effect on genuine fire-damp; and a stream of gas burning in the atmosphere, acting on a small quantity of matter, is entirely different from an explosive mixture, which is uniform within the lamp."

But the more serious charges against the lamp are the explosions likely to be occasioned by the fine coal-dust, &c. floating in the atmosphere of the mine; for against this evil, no caution could provide a remedy; whereas, from what has been stated, it is evident, that most extraordinary carelessness must co-operate with most extraordinary circumstances, to make an explosion possible from the mere flame of the fire-damp burning in the lamp. We must therefore inquire how correct the experiments of Mr. Holmes are; and we shall see presently, that when

when fairly stated and rationally investigated, not one of
them militates in the slightest degree against the perfect safety
of the lamp.—And here it may very probably be urged, and
with perfect truth, that all this was known before, from Sir
Humphry Davy's own experiments, and consequently that mine
are quite superfluous, and readily admit it. Sir Humphry, at the
first suggestion that was apprehended by the miners from
the coal-dust, immediately submitted the lamp to rigorous trials,
to prove how well or ill their fears were founded. He repeatedly
threw coal-dust, powdered rosin, and witch-meal, through
lamps burning in more explosive mixtures than ever occur in
coal-mines; and though he kept these substances floating in the
explosive atmosphere, and heaped them upon the top of the
lamp when it was red hot, yet he never could communicate ex-
plosions by means of them. Phosphorus and sulphur only pro-
duced that effect by being applied to the outside of the lamp,
and even they required to be in large quantities and *blown upon
by a current of fresh air.*—Vide Philos. Magazine, vol. xlviii.
p. 54. The liberal and enlightened will not be disposed to
question the truth of this statement, from one whose candour
and accuracy are equally acknowledged; and to such all further
evidence is work of supererogation,—to them I do not write:—
but we have seen, unhappily, that all are not of that class; and
though I do not hope to reclaim the perverse and malicious, I
notwithstanding am anxious, as far as my feeble efforts can assist,
to prevent others really desirous of the truth, from being misled
into the path of error and unfounded scepticism. Feelings of
attachment towards a man whom I glory in calling my friend,
cannot but create in me a warm interest for all that tends to
exalt the lustre of his well-acquired fame;—but beyond the feel-
ings of friendship, or the fame of my friend, I am anxious for
the prevalence of truth, and that one of the greatest benefits
science ever conferred on humanity should shed the full influence
of its blessing, spite of the malicious insinuations of ignorance
and envy.—I return to the experiments.

Exp. 1. I suspended the lamp in my lanthorn and threw in
the coal-gas from a bladder as before; and when the gauze cy-
linder was filled with flame, a mixture of about three parts of
finely levigated coal-dust and one part of gunpowder-dust was
thrown into the lanthorn, which occasioned an abundance of
sparks within the gauze cylinder, but no explosion of the ex-
terior gas.

Exp. 2. I fixed a small tray of very thin copper-plate, con-
taining the mixture of coal-dust and gunpowder, about three
inches above the wick of the lamp within the gauze cylinder,
suspended the lamp in the lanthorn, and threw in the gas as

before.   The mixture took fire in the tray as soon as enveloped
by the inflamed gas, and burnt with a very large flame mixed
with innumerable scintillations; but no explosion (though the
experiment was continued till all the gas of a very large bladder
was expended) could be produced.

Exp. 3. The lamp and lanthorn being arranged as before, the
gas was thrown in at one of the lower apertures, and the
coal-dust and gunpowder at an upper and a current of air
forced on the inflamed gas from another bladder through the
opposite middle opening.   The whole burned with a very full and
strong flame, and the scintillations were so violent as almost to
resemble the tail of a sky-rocket, and the wire-gauze soon be-
came red hot, but no explosion ensued.   Had I continued the
experiment till the wire-gauze became nearly white hot, an ex-
plosion of course would have followed; but it would take place
as certainly without the coal-dust.   It is therefore fairly esta-
blished that coal-dust cannot communicate flame to the external
air, when the lamp is immersed in the most explosive mixtures.

I repeated the last experiment with the double lamps, and also
without the coal-dust, and used every endeavour to produce an
explosion, but without effect.   I raised the wire-gauzes to a dull
red heat, but could not get them hot enough to suffer flame to
pass through.   In every case possible, therefore, the double lamp
is perfectly secure; and, unless in the hands of an idiot or a mad-
man, the single is hardly less so;—nothing but a current of air,
directing the flame with considerable force to one point, can
heat the gauze sufficiently for flame to traverse it;—and should
such a current be met with in the mine, it must inevitably ex-
tinguish the lamp, and thus be itself the safeguard from its own
danger.

Mr. Holmes, however, caused explosions by holding the lamp
over a small gas tube, and projecting coal-dust and gunpowder
and even coal-dust alone, on it; and I understand the same ef-
fect has been produced in the laboratory of the Royal Institution.
In those experiments, I am told the lamp was held nearly hori-
zontally over the pipe of the large gasometer (and consequently
the full heat of the flame from the wick of the lamp con-
stantly directed to one point), and coal-dust thrown on the top
of the gauze.   Mr. Holmes does not state in what direction he
held his lamp.   In this arrangement it is obvious that a current
of fresh air round the lamp is established, the coal-dust is
heated red by contact with the wire-gauze, and its ignition in-
creased by the current of air; and thus it reaches the tempera-
ture at which it can explode the circumambient gas; but this
will not happen without that partial current, which can never
prevail in the mine, nor I believe will it happen at all, if the lamps
                                                                    be

be held vertically; at least I made repeated attempts to produce
an explosion with the lamp in that position, the gas being
thrown into it in the free atmosphere in vain, although I kept
the whole cylinder a considerable time completely filled with
flame, and the powdered mixture constantly projected into it;
but when inclined, succeeded. A mixture of gunpowder with
the coal was however necessary; without it I could obtain no ex-
plosion, nor with it could I always succeed; *never*, indeed, till
after the gas had been thrown in long enough to heat the wire
very hot; but,—and it proves the accuracy of the reasoning on
the experiment,—when once an explosion was effected, it might
be repeated at pleasure, provided the gauze cylinder was not
suffered to cool. With the double lamps I could not by any
means produce an explosion, whatever position it was held in.

I placed coal-dust and gunpowder on the top of the wire-
gauze cylinder suspended in the lanthorn, and threw in the coal-
gas. The whole cylinder was filled with flame, and the upper
part became red hot. The mixture burned, and threw off sparks,
but occasioned no explosion. On putting a lighted match into
the lanthorn the gas immediately exploded with violence.

The same experiment was repeated with the addition of a jet
of atmospheric air thrown from a bladder with a small blow-
pipe on the ignited coal and powder dust—but neither in this
case did any explosion ensue.

A few words more, and I have done. Mr. Holmes seems in-
clined to hint that Sir Humphry Davy has borrowed the idea of
his lamp from Stevenson's, and talks of the " *alteration*" he has
made on it, " by weaving the holes instead of having them
punched." If Mr. Holmes were acquainted with the progress
of the investigation by which Sir Humphry was led, step by step,
from successively established facts to the present perfection of
the instrument, he would know that Sir H. borrowed no idea
concerning it from Mr. Stevenson or any one else. It was early
in last October that Sir Humphry communicated in confidence
to me the discovery of the principle which was the base of his
subsequent reasonings, viz. the narrow limits in which the pro-
portions of atmospheric air and fire-damp can be combined so
as to afford an explosive mixture;—and on the following day
he showed Mr. Brande and myself the experiments which con-
firmed its accuracy. Like every thing else that Sir Humphry
has done, this lamp is the result of consequences most sagaciously
deduced from causes most ingeniously and diligently inquired
into;—and it is to be borne, that the labours of such a man are
to be made the subject for every envious caviller to vent his spleen
on, or that it should be insinuated, that their results are pla-
giarisms and offerings, from the accidental (however happy) dis-

covery

covery of one who (confessedly even by Mr. Holmes himself) is ignorant of the principles of his own invention?

Mr. Holmes's last paragraph is calculated only to excite feelings I will not indulge in, and I shall not give it any other notice than to observe, that I believe the assertions it contains are as inaccurate as they are illiberal,

I have the honour to remain, sir,
Your very obedient servant,
JOHN GEORGE CHILDREN.

P. S.—I should have made this communication earlier, had not absence from home obliged me to defer the experiments till too late for its insertion in your last number.

XXXIX. *On certain Experiments with Sir H. Davy's Safe-lamp, reported to have been made at the Gas Establishment in Dorset-street. By* WILLIAM KNIGHT, *Esq. the Manager of that Establishment.*

To Mr. Tilloch.

SIR, —I HOPE you will permit me, through your excellent publication, to endeavour to remove any unfavourable impression which may have been made by a paper on Sir H. Davy's Safety-lamp, in the August Number of the Annals of Philosophy, by a Mr. Holmes, relative to some experiments conducted at the Gas Establishment in Dorset-street, with which I am connected.

Mr. Holmes speaks of a visit to the works for the purpose of trying experiments on Sir H. Davy's safety-lamp, and mentions persons who were present at the trials. Does Mr. Holmes publish their names, to attach importance to his liberal investigation? If he does, it is fair to ask what credit his experiments derive from the presence of Mr. Wheatcroft, who is employed here to pay the workmen; Mr. Morris, who superintends the works by night; and Mr. May, of whom I never heard before he obtained importance in Mr. Holmes's letter. How Mr. Holmes had access to the works, I know not; nor would I or any other proprietor knowingly lend ourselves, or the establishment, to experiments not openly conducted. Whenever gentlemen, who are men of science, have wished to visit the works, an application has been usually made to me as engineer, and I have always been ready to afford free inspection, and give every facility to experiments; but in this instance I never heard of Mr. Holmes or his visit till I saw his own report in the Annals of Philosophy, and that only yesterday, when the last number was put into my hands. As the active partner and manager of the establish-

establishment, and having █████████ and credit more immedi-
ately connected with the wo██████████y other proprietor, I feel
myself called on to disclaim.p███████n in any experiments con-
ducted there by Mr. Holmes, █████ so opposite to those of
other persons, who with liberal██████ have felt anxious to prove
the security which Sir H. D██████iscovery promises, and with
the benevolent hope of its app██████tion to the safety of the mem-
bers of a valuable class of soci████ After what I have seen and
heard of Sir H. Davy's lamp, my conviction of its security is
not shaken by Mr. Holmes's report, which looks more like an
endeavour to serve his friend Dr. Clanny than the public.

Any means which our 'works in Dorset Street can afford to
scientific and liberal men, of investigating further the applica-
tion of Sir H. Davy's or any other discovery, will be granted
with pleasure by, Sir,

Your obedient humble servant,

City Gas Works, Dorset-street, Salisbury-sq.   WM. KNIGHT.
      London, 29th August, 1816.

XL.   *Suggestions arising from Inspections of Wire-gauze
       Lamps, in their working State, in Mines. By* Sir H. DAVY.

THE inspection of a number of wire-gauze safe-lamps, that have
been long in common use in coal mines, and the examination
of the effects of different explosive atmospheres of fire-damp
upon them, enable me to offer a few suggestions, which I hope
will be of use to the miner.

The wire-gauze cylinders ought never to be taken out of the
screw-piece in which they are fixed; and in the lamps con-
structed at Newcastle, which have not the same rim of wire-
gauze as those of Newman's construction, the wire-gauze ought
to be soldered to the screw-piece, or fixed to it by rivets.

The wire-gauze is easily cleaned without being detached, by
a brush of the same kind as that used for cleaning bottles, and
one of these brushes ought to be furnished with every lamp.

The wire-gauze in several lamps in the collieries, which had
been in use six months, and cleaned by careful workmen with-
out being removed, was as good as new; whereas the gauze in
some, that had been used for a much shorter time, and taken
out of the lamp and cleaned roughly, was injured at the bottom,
and, if not actually unsafe, becoming so.

In one instance, I found a lamp which had been furnished to
a workman without a second top. This is a gross and unpar-
donable instance of carelessness in the maker, who, if any acci-
dent had happened, would have been guilty of homicide.

                          N 3   All

All the lamps that I ha   mined have   different times
been red hot; and a work   at the Hebburn colliery showed
me a lamp, which, though it had been in use about sixteen hours
a-day, for nearly three months, was still in excellent condition :
he also said it had been red hot sometimes for several hours
together. Wherever workmen, however, are exposed to such
highly explosive mixtures, double gauze lamps should be used,
or a lamp in which the circulation of air is diminished by a tin-
plate reflector placed in the inside, or a cylinder of glass reaching
as high as the double wire, with an aperture in the inside; or
slips of Muscovy glass may be placed within the lamp, and in
this way the quantity of fire-damp consumed, and consequently
of heat produced, may be diminished to any extent. Such lamps
likewise may be more easily cleaned than the simple wire-gauze
lamp; for the smoke may be wiped off in an instant from the
tin-plate or glass *.

If a blower or strong current of fire-damp is to be approached,
double gauze lamps, or lamps in which the circulation of air is
interrupted by slips of metal or glass, should be used ; or, if the
single lamp be employed, it should be put into a common horn or
glass lanthorn, the door of which may be removed or open.

The wire gauze is impermeable to the flame of all currents of
fire-damp, as long as it is not heated above redness ; but if the
iron wire be made to burn, as at a strong welding heat, of course
it can be no longer safe; and though such a circumstance can
perhaps never happen in a colliery, yet it ought to be known and
guarded against. And if a workman, having a single lamp, should
accidentally meet a blower acting on a current of fresh air, he
ought, on finding his lamp becoming hot, to take it out of the
point of mixture, or screen it from the current.

I have had an excellent opportunity of making experiments on
a most violent blower, at a mine belonging to J. G. Lambton, esq.
some of them in the presence of Mr. Lambton : in most of them
Mr. Buddle assisted. This blower is walled off from the mine
and carried to the surface, where it is discharged with great
force. It is made to pass through a leathern pipe, so as to give
a stream, of which the force was felt at about two feet from the
aperture in a strong current of air. The common single working
lamps and double gauze lamps were brought upon this current,
both in the free atmosphere and in a confined air. The gas fired
in the lamps in various trials, but did not heat them above dull
redness, and when they were brought far into the stream they
were finally extinguished.

* Models of the different modifications of the wire-gauze safety-lamps
may be seen at the shop of Mr. Coxe, brass-founder, in Gateshead, by
whom they were made.

A brass pipe was now fixed ▓▓▓▓ blower tube, so as to make the whole stream pass through the aperture of less than half an inch in diameter, which of course formed a most powerful blow-pipe, from which the fire damp, when inflamed, issued with great violence and a roaring noise, making an intense flame of the length of five feet. The blowpipe was exposed at right angles to a strong wind, and the double gauze lamps and single lamps successively placed in it. The double gauze lamps soon became red hot at the point of action of the two currents; but the wire did not burn, nor did it communicate explosion. The single gauze lamp did not communicate explosion, as long as it was red hot and slowly moved through the currents; but when it was fixed at the point of most intense combustion, it reached a welding heat, the iron wire began to burn with sparks, and the explosion then passed.

In a second and third set of experiments on this violent blow-pipe of fire-damp, single lamps, with slips of tin-plate on the outside or in the inside, to prevent the free passage of the current, and double lamps, were exposed to all the circumstances of the blast, both in the open air and in an engine-house where the atmosphere was explosive to a great extent round the pipe, and through which there was a strong current of atmospheric air; but the heat of the wire never approached near the point at which iron wire burns, and the explosion could never be communicated. The flame of the fire-damp flickered and roared in the lamps, but did not escape from its prison.

There is no reason ever to expect a blow-pipe of this kind in a mine; but, if it should occur, the mode of facing it and examining it, with most perfect security, is shown; and the lamp offers a resource, which can never exist in a steel-mill, the sparks of which would undoubtedly inflame a current of this kind.

Arguments have been stated as to the weakness of the lamps. In a board or gallery in the Wallsend colliery, Mr. Buddle and myself, with some of the viewers, endeavoured to injure a single-gauze lamp by throwing large pieces of coals upon it, and striking it with a pick; but we never perforated the gauze; and the lamp, after these severe trials, burnt with perfect security in a small explosive atmosphere made by Mr. Buddle at the bottom of the shaft for the purpose of trying the lamps.

I made with Mr. Buddle and his viewers some experiments on the comparative light of the lamps, the common miner's candle, and the steel mills, in a gallery in the Wallsend colliery. We judged of the intensity of the light by the square of the distance at which a small object was visible; and made repeated trials on each species of light.

The

The light of the miner's c[...]   -   45 5,
That of a lamp furnished w[...]tin plate reflector for di-
   minishing the circulation of the air, and facing a
   blower, was   -   -   -   -   49.
That of a single common lamp   -   -   39.
That of a double copper wire lamp   -   -   25.
That of the steel mill, very unequal and uncertain ; but
   at its greatest intensity of light   -   -   -   25.

It may be proper to observe, without reference to the supe-
riority of light, that coals may he worked nearly twice as cheap
by the wire gauze safe lamp, as by the steel mill.

The pleasure of seeing the wire gauze safe lamps in general
use amongst the miners, and adding to the security and happi-
ness of this useful class of men, amply repays me for the labour
of twelve months devoted to their cause, and for the anxiety
which I have often experienced during the progress of the in-
vestigation.

Newcastle, Sept. 9, 1816.   H. DAVY.

P. S.—I have shown in a paper printed in the Transactions of
the Royal Society, that the power of heated wire-gauze, to permit
the passage of the flame of coal-gas, is directly as the size of the
apertures, and, to a certain extent, as the velocity of the current:
I say to a certain extent, because, by a current of a *certain velo-
city*, flame is extinguished. A very slight motion will pass the
flame of coal-gas through wire-gauze having less than 400
apertures to the square inch, even when it is heated to dull red-
ness ; but a very strong current and an ignition above redness,
visible in day-light, is required to pass the same flame through
wire-gauze having above 700 apertures to the square inch: and
I have never been able to pass the flame of coal-gas or any car-
bonaceous flame through wire-gauze having more than 1600
apertures to the square inch, by any means.

The experiments above detailed on the blower, are the first I
have made upon *currents of fire-damp*. They prove what I had
inferred from its other properties, and they offer simple means
of rendering wire-gauze lamps perfectly safe against *all* circum-
stances, however extraordinary and unexpected, and of placing
their security above the possibility of *doubt or cavil*.

XLI. *On the Cosmogony of M———n reply to* Dr. Prichard. *By* F. E———s.

To Mr. Tilloch.

Sir, — Doctor Prichard confesses that he ascribed to me a declaration I had not uttered, and which *he never supposed* I meant to make. Neither this confession, nor his ingenious attempt, by what he calls " *synonymous expressions,*" to extract from my words something equivalent to the imputed declaration, needs any commentary. Nor do I deem it necessary to offer more than one or two short observations on the other parts of his last communication.

Although obliged to abandon the exclusion of the *whole* order of testacea from the fifth day's creation, Doctor Prichard still considers his *coincidences* safe, so long as it is not demonstrated that the portion of the order which he persists in excluding, enjoyed the power of creeping, or progressive motion. It would seem, however, that it was incumbent on him to have proved the want of this species of motion in the excluded testacea, before he assigned it as the reason of their exclusion. But even the accomplishment of this not easy task* would avail him little : much less equivocal distinctions than that founded on particular modes of motion, preclude testacea from a place in the *third day's creation*, in which *nothing in the waters*, nor any thing *animate* on the land, is said to have been called into existence. It will scarcely be contended that between *land* and *water*, between *animate* and *inanimate*, the distinctions are not infinitely wider and more important, than between *dissimilar modes of animal motion*. Nor can it be maintained that the *testacea* which the *coincidences* of Doctor Prichard require to be excluded from the *fifth* and included in the *third day's creation*, do not bear an incomparably closer analogy to those *testacea*, which he is obliged to acknowledge cannot be excluded from the *fifth day*, than they do to the *grass, seed-bearing herbs*, and *fruit-bearing trees* of the *third day's creation*.

Here I am willing the discussion should close, being content to submit to the decision of those who may have attended to its origin and progress, whether I have exhibited the captious cavilling spirit, and other estimable qualities, with which the liberality of my opponent has endued me. I am, sir,

Your very obedient servant,

Bath, 11th ——— 1816.   F. E———s.

* Had ——— (for instance) happened to be what is, perhaps not very prop——— termed an oceanic shell, it may be presumed that from its inspection Dr. Prichard never could infer the power of progressive motion which ——— valve is known to enjoy.

XLII. *On Magnesio-Sulphate of Soda. By Mr. J. Heales.*

To Mr. Tilloch.

Sir,—A communication, I observe, has been presented to the Royal Society of Edinburgh, by Dr. Murray, on the analysis of sea water, the first part of which was read April the 15th, and the conclusion on the 20th of May last. It is there shown by this excellent chemist, that the brine or mother-liquor of sea-water abounds with a salt, which, he says, " seems hitherto to have escaped observation. It is a compound of the sulphates of magnesia, and of soda, which together crystallize in very regular rhombs, occasionally truncated on some of the edges and angles ; and this compound salt contains a much smaller quantity of water of crystallization than either of the sulphates of which it is formed, is less disagreeable to the taste ; differs from both in all its other properties; and has not hitherto been applied to any useful purpose, but may probably form a very excellent purgative salt *."

This aperient salt has certainly been long in use, although we find no exact account of its qualities or any description of its composition published. It was sold, I am informed, many years ago under the name of *Lymington* Glauber's salt ; it was then considered as an *adulteration*, and therefore brought an inferior price in the market. At that time, the true Glauber's salt was very costly, and chiefly obtained in the process for muriatic acid ; but the improved methods of preparing the crude muriate of ammonia have greatly reduced the value, so much so that the Lymington salt is no longer sold as a *substitute* for Glauber's, or the sulphate of soda.

Whether the extensive works at Lymington furnished this salt simply by evaporating the brines, or by mixing the two sulphates in some certain proportions, I cannot determine ; I have, however, seen an old prescription for preparing it, which consists in mixing two parts of sulphate of soda with one of the sulphate of magnesia, and in the usual way proceeding to crystallize the product.

That this compound salt, which for the present may be called the *magnesio-sulphate* of soda, is the chief ingredient in many of the natural saline aperient springs, and gives the purgative quality to sea-water, I have scarcely any doubt; and when its composition shall have been more attentively examined, we shall probably find that in every case it will consist of *definite* proportions of its three components,—sulphuric acid and the two bases, viz. soda and magnesia.

The analysis of a mineral water, as Dr. Saunders justly observes, had better be incomplete than inaccurate, and Dr. Fothergill's experimental inquiry concerning the Cheltenham

* New Journal of the Royal Institution of Great Britain, vol. i. p. 204.

water, although incomplete, is however valuable that it describes the *double* sulphate as *one* salt, while all other analytical writers have stated the quantities of sulphate of soda and that of magnesia separately.

Perhaps the more accurate statement would be to estimate first, how much of the triple salt exists, and then put down the quantity of that sulphate, whether of soda or magnesia, which is in excess.

In the analysis of the Kilburn water, both by Mr. Bliss and Mr. Schmeisser, the two sulphates are estimated as existing separately; which cannot be correct, since in their mixed state they possess new and peculiar properties. These gentlemen do not exactly agree in the estimate of the two salts; this difference may however have been occasioned by one of them not making his analysis at the same season of the year.

As the subject is now in such very able hands, we shall probably soon be favoured with Dr. Murray's analysis of this very useful and commodious purgative salt; I shall therefore forbear entering upon particulars at this time. Still, however, I must notice that it is an article of which I have long had some knowledge, and have witnessed its peculiar properties, having had, during the five last years of my residence with Mr. Hume of Long Acre, very frequent opportunities of observing many of its habits as a chemical composition, and also of its efficacy as a medicine. That a mixture of the sulphate of soda with that of magnesia requires less water of crystallization, and that the compound salt proves more purgative than either of the two constituents in the same proportion, were, among other remarks, first noticed to me by Mr. Hume. I know he has long ago sold and recommended this salt to many of the first professional men in London, and urged its being admitted into the pharmacopœia as a standard remedy.

This salt, in the proportion of about one ounce or more to a gallon of water, forms the best substitute for Cheltenham water. There can be no chalybeate property obtained from the true Cheltenham water so as to be *soluble* and useful in the salt remaining after evaporation, notwithstanding the plausible and repeated advertisements which endeavour to insinuate to the contrary; for, the iron or, as it is popularly termed, steel with which nature has impregnated many mineral waters is nearly always held in solution by *carbonic acid;* this compound is therefore instantly destroyed by evaporation. But when the addition of a chalybeate is required, there are many obvious ways of effecting that purpose, of which no medical man can be ignorant, and such variations, when made with judgement, will often produce the happiest effects. I am, sir,

Yours respectfully,

J. HEALES.

Burton Crescent, Sept. 14, 1816.

XLIII. On Vulgar Fractions.

To Mr. Tilloch.

Sir, — In your Magazine for May, are some remarks by Mr. Farey senior, upon what he considers a singular property of vulgar fractions, but which upon minute examination will be found nothing more than what is deducible from the rules of arithmetical proportion.

No fractions can be added, without first being reduced to a common denominator, and then the numerator is only increased in quantity, the denominator remaining as before; but upon Mr. F's proviso, both numerator and denominator are individually added to the corresponding ones, and that without being reduced to a common value; consequently we must refer to the integers which compose the fractions, and not to the fractions collectively, for the properties Mr. F. has pointed out.

Now, if any four numbers be in arithmetical proportion, the sum of the means will always be equal the sum of the extremes, $e. g.$ If we take the numbers 5, 4, 3, 2, then will $5 + 2 = 4 + 3$: or in the numbers 5, 4, 4, 3 (which are still in proportion, though the two means are equal, for the difference between the 1st and 3rd is equal to the difference between the 2d and 4th), $\therefore 5 + 3 = 4 + 4 = 4 \times 2.$ Now, if we take the fractions $\frac{1}{4}, \frac{1}{4}, \frac{1}{3}$, which are a part of Mr. F's series, it is evident $5 + 3 = 4 \times 2$; and the numerators being equal, $1 + 1 = 1 \times 2$; or $\frac{1+1}{5+3} = \frac{1 \times 2}{4 \times 2} = \frac{2}{8}$, which is of the same value as $\frac{1}{4}$, both terms being multiplied by the like common number: or if we take the numbers $\frac{2}{5}, \frac{1}{2}, \frac{3}{5}$, the result still agrees with the principles laid down: for, if the fractions are reduced to a common denominator, the series will be $\frac{4}{10}, \frac{5}{10}, \frac{6}{10}$, where the numerators only will be in progression, and the denominators equal: or, if we take part of the series $\frac{15}{52}, \frac{28}{97}, \frac{13}{45}$, &c. $= \frac{30}{104}, \frac{28}{97}, \frac{26}{90}$, &c. both terms are in regular progression: therefore $\frac{30+26}{104+90} = \frac{56}{194}$, and so on, for any other series laid down according to the rule given.

I am, sir, yours respectfully,

Wakefield, August ... 1816.   S. A.

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XLIV. *Observations on the Hypothesis of some modern Writers,
that America has been peopled by a distinct Race of Men and
Animals; with some Proofs arising from the Natural History
and Appearances of the new Continent in favour of the
Mosaic Account of the Deluge.* By HUGH WILLIAMSON,
M.D. of New-York* .

W E observe a regular systematical change in the colour, shape,
and features of men, to the north and the south. From the
climate of a fair skin, fine shape and pleasing feature, going to
the northward, the skin becomes of a blackish brown, the figure
clumsy, and the features coarse. Going to the southward, in the
same manner, we alter the complexion, shape and features, until
the skin becomes perfectly black, the shape in some countries
less graceful, and the features coarse: the colour being alteied,
according to the soil, situation and climate, by the most regular
and insensible deviations and shades.

Those facts being considered; it being also observed, that
every change is most proper and best adapted to the climate, or
that it is the natural effect of such climate; there can be no
moral or physical proposition more certain, than that all those
people are descended from the same family.

The philosophers, who discovered several races of men on the
old continent, have not failed to plant a new and distinct race
of men in America. In support of this opinion, they allege that
the American Indians do not differ from one another in colour,
like the inhabitants of the other continent† : their colour also
is different from that of any other people: that the American
has no beard; that he is more frigid, more weak and more
cowardly than the inhabitants of the old continent.

This humble and subordinate character of the American sa-
vage has not always been urged as a direct proof that he be-
longs to a separate race of men, for it has occasionally been ad-
vanced in the pride of country; a species of pride that will not
suffer children to equal their ancestors; that makes it impossi-
ble for them to obtain such equality, because there is something
in America, as they allege, "that is less favourable to the
strength and perfection of animal creation."

The complexion of the American savage, or the sameness of
colour that is observed among those people, forms the most re-
markable trait of their character. When we observe, in the old

* From "Observations on the Climate in different Parts of America,
compared with the Climate in corresponding Parts of the other Continent."
New-York, printed 1815.
† Raynal's Phil. and Polit. Hist.

continent,

continent, all the varieties of shades, from perfect white to perfect black, we are naturally surprised that in the new world, which extends to a higher degree of north and south latitude, including every habitable region, there should not be a black man, nor one, as it has been alleged, who is perfectly white. The natives are generally of a reddish brown. Their colour seems to be a mixture of white and black, reddened by paint, or by the blood appearing through the skin, which is not thick. This again receives a brownish cast by more or less exposure to the weather.

On the whole continent of America, there is not a black Indian, nor is there a spot for which a black skin is required. No winds prevail in America that rise on a hot surface or a sandy desert; nor is there any large tract, within the tropics, that is remarkably hot. The greater part of this continent is divided by a long chain of mountains, that extends from north to south. These mountains, the highest in the world, have an astonishing effect upon the climate, on both sides of the continent. They lie across the trade winds, and cut them off; for they rise above the winds. They are generally distant about seventy or eighty miles from the Pacific Ocean, within the tropics; but the whole space between those mountains and the Pacific Ocean is so far from being parched by a hot vertical sun, that the inhabitants enjoy the most pleasing temperature. There is a sandy desert, nearly one hundred miles in extent, between Sachara and Lima, about the seventh degree of south latitude. Such an expanse of dry sand, under a vertical sun, in any part of the other continent, would produce great heat, and give a sable colouring to the people in its vicinity. But in the province of Lima it can produce no such effect, because the wind in those regions ought to blow from the east; but there are mountains in that direction, at no great distance, covered with perpetual snow.

The trade winds to the eastward of the Andes are checked by those mountains; there they deposit all the water with which they had been charged. The quantity of rain in that region being great, the process of evaporation must also be great, whereby the heat of the atmosphere is moderated. A reddish brown, with a tawny cast, is the darkest colour that can be expected in such a climate. America, on both sides of the Andes, above the tropics should produce, as in some parts of the old continent, in similar latitudes, a brown or dusky set of men, until we reach a high degree of latitude; and it is very questionable, whether a race of men, perfectly fair, will ever be found to preserve that complexion for many ages in any part of America, to the eastward of the Cordilleras; except in high latitudes, and

near

near the coast. There are not any people, on the old continent, perfectly fair, except those who live in high latitudes, where the westerly winds come from the sea, at no great distance, so tempered as not to be very sharp nor very dry. This rule applies to Great Britain and Ireland, to the Germans, Danes, Swedes, and Circassians *; but going to the eastward in the same latitude, as we depart from the ocean or the Black Sea, having more dry land to the windward, by which the air is charged with sundry exhalations, the skin changes its colour; it ceases to be perfectly fair. There is not, in the eastern part of Asia, between the extremes of heat and cold, a nation perfectly fair. The best complexions are found near the head of the Ganges, among the mountains of Thibet. We may discover a concurrence of circumstances, in the British isles, and near the German Ocean, not found in many other places, which are necessary to a fair skin. They are little exposed to the warm sun; they have little intense cold, and their winds usually come from a watery surface. Their westerly winds are from the ocean, and their atmosphere is loaded with moisture. They have not much rain, but their showers are of long continuance; they have much dark cloudy weather, and the rays of the sun are feeble when he visits the inhabitants. They never experience that warm clear sun, which freckles or tans the skin; nor those long intense colds, which injure the cutaneous nerves, and produce a reddish brown. While America remained a great forest, inhabited by savages, under the constant dominion of westerly winds, there was not any climate on the eastern coast in which we could expect a fair skin. By the progress of cultivation, the general course of the winds is materially affected in the middle and northern states; and in the process of time we may expect such a prevalence of easterly winds, near the coast in those states, as shall prevent that tendency of complexion to the clear brunet, which prevails in temperate climates, in other parts of the world.

Although no part of America is fitted to the production of a black skin, nor would many parts of this continent be expected to produce a skin perfectly fair, among the original inhabitants; we are not to believe, as some writers have alleged, that the American Indians are all of one colour. Their skin is tinged with a variety of shades between white and black, but there are Indians, as we are told, above the latitude of 45 degrees north, who are nearly white; and there are Indians in Guiana and Brazil, at a distance from the coast, whose skins are very dark,

* London in latitude 51°, P----- 50°, Copenhagen ----, Circassia 45°, having the Black Sea and the ----- Asia to the south-west and north-west.

I will

I was informed by the Little Turtle, who is a chief of the Miami tribe of the lakes, and has an extensive acquaintance with the Indians, that the northern Indians are much fairer than those who live in warm climates; except that Indians, who live near the lakes, and are much exposed to the sun, in fishing and swimming, have darker skins than other northern Indians. He understands that Indians who live northward from the sources of the Mississippi, are fairer than those of his own nation who live in the opposite direction.

The Indians at Matagrassa, as we are told by Condamine, are of different shades, according to the elevation of the country, some of them being almost fair *. The testimony of Molina is also very explicit on this subject. " The natives of Chili form but one nation, that is divided into various tribes, who have a similar physiognomy, and speak the same tongue, which may be called the Chilese language. It is soft, harmonious, regular, and abounding in words that in all cases are fit to express not only physical but moral and abstract ideas. Those people are of a brown coppery colour; but the Boroani, who are situated in the centre of the province of Arauco, in the thirty-ninth degree of south latitude, are white and red, with blue eyes and fair hair, like the Europeans, who are born in the middle of the northern temperate zone. Their features are regular, and some of them are beautiful †."

When South-America shall be well cultivated, the timber cut down, the quantity of rain diminished, stagnant pools dried, and the rivers contained within their proper banks, the easterly winds being checked by the warmer surface of cultivated lands, a dusky race of men, nearly black, are to be expected in Brazil, about the latitude of Cape St. Roque; for that is the only part of America in which the progress of industry may darken the skin, notwithstanding the effects of civilization.

As no proof can be given, that the American Indians are a new race of men, I shall consider the other trite allegation, that " animal nature degenerates in America." This opinion, advanced by the eloquent Buffon, and supported by many arguments, has also been repeated by Dr. Robertson, the Abbé Raynal, and by other writers. The most remarkable appearance is that " all animals in America, including those who have been naturalized to the climate, are comparatively inferior in size to those of the old continent. Nature appears in the new world, to have finished her works upon a smaller scale."

* Voyage de Condamine.
† Compendio de la Historia geografica naturaly civil del Regno de Chile. Por el Abate Don Juan Ignatio Molina. I have not seen the Italian original, but I presume that the Spanish translation is correct.

" There

" There seems therefore to be, in the combination of elements, and other physical causes, in this new world, something that is opposed to the amplification of animated nature. There are some obstacles to the development and perhaps to the formation of great germs.

" Although the savage of America is nearly of the same stature with men in the other continent, this is not a sufficient exception to the general contraction of animated nature through that whole continent. The American savage . . . . . . has no hair, no beard, no ardour for his female. Though nimbler than the European, because he is more accustomed to running, his strength is not so great. His sensations are less acute, but he is at the same time more timid and cowardly. He is without vivacity or enterprise *."

" America gives birth to no creature of such bulk as to be compared with the elephant or rhinoceros, nor that equals the lion or tiger in strength and ferocity. The same qualities, in the climate of America, which stinted the growth and enfeebled the spirit of its native animals, have proved pernicious to such as have migrated into it voluntarily, from the old continent, or have been transported hither by the Europeans.

" Most of the domestic animals, with which the Europeans stored the provinces when they settled there, have degenerated with respect to bulk and quality, in a country whose temperature and soil seem to be less favourable to the strength and perfection of animal creation †."

The whole of this description is poetical and imaginary; for it has no foundation in nature. It is not from any vice in the climate, nor the want of proper food, but from the happy state of our country, from the general ease with which men have supported themselves in America, that domestic animals have been supposed to degenerate. Nothing less than necessity has ever produced diligence in any kingdom or state. The man who has little to do, acquires habits of idleness, and he does less. In Europe, where the means of living are difficult, pasturage scarce and forage dear, the farmer is restrained in the number of his cattle; for this reason the cattle he keeps are attended with great care. They are duly housed and fed; the largest and best are preserved for breed, and every thing is done by which the size may be increased and the value enhanced of the few he has for use. The forest, in America, supplied the stock with pasture during the summer, and during the winter, in some of the colonies when they were first settled. In the northern colonies, the cattle were within winter, but they were seldom

housed. Hence it follows, that they were shrivelled and di-
minished, by cold storms, hail and snow, as the human species
have been diminished in Lapland and Siberia. In addition to
those diminishing causes, the first colonists, in most cases, were
inattentive to the size of the male or female from which their
cattle were to spring. We have a remarkable instance, in the
Chickesaw nation, of the bad effects of breeding from diminutive
parents. Those Indians were originally furnished by De Soto
with a breed of Spanish horses*. In that country the horses
provided for themselves, the soil being good and the climate
warm. The Indians, towards the middle of the last century,
discovered that their horses were a valuable article of commerce;
they could be exchanged for guns, blankets, and other neces-
saries; but the traders, in all cases, bought the largest horses,
and the smallest were left to continue the breed. The effect is
obvious, for the Chickesaw horses are confessedly smaller than
they were fifty years ago. Other causes, sufficiently numerous,
may be given of quadrupeds degenerating in America, under the
shrivelling hand of indolence and neglect; but it would not fol-
low, from a thousand such examples, that America cannot pro-
duce a race of animals large and vigorous as similar animals in
the old continent. I do not say that America has produced
greater or stronger animals than ever were seen on the opposite
part of the globe, but we know that bones have been found,
both in North and South America, of sundry animals, granivorous
and carnivorous, that were greatly superior in size to the ele-
phant, the lion, or any other beast now living in the old conti-
nent. Although the beast, whose bones and claws were lately
found in Green-Brier, in Virginia†, must have been a carnivorous
animal, and greatly superior to the lion in strength, we cannot
affirm that he was equally fierce; for it is admitted, that lions
who are found near mount Atlas are neither so fierce nor strong
as those which are nourished on the burning deserts of Nigritia.
From this we infer, that extreme heat conduces to the ferocity
of beasts of prey, and that animals of the carnivorous kind are
less ferocious in America than in the hotter regions of the other
continent. With respect to our domestic animals, whose parents
have been imported from Europe, we should not boast in our
turn, by saying that the present race is larger or stronger than
those who were imported; but we may affirm, without danger
of being refuted, that there are numerous instances of cattle,
lately raised in the United States, full as large as any of the same
kind in Europe. If it should be alleged that animals frequently

* De Soto passed a winter among the Chickesaws, near the river Missis-
sippi, and left some of his horses there.
† See Transactions of the American Philosophical Society, vol. iv. p. 246.

improve

improve under the influence of our happy soil and climate, we might quote an author of great reputation, who lived in Europe, in favour of that position *. Speaking of Chili in South America, he says, "The animals of our hemisphere not only multiply, but improve in this delightful region. The horned cattle are of a larger size than those of Spain. Its breed of horses surpass, both in beauty and spirit, the famous Andalusian race from which they sprang."

Does the human race degenerate in America? We are much interested in this question, whatever the fate of quadrupeds may be. The want of beard, in the American savage, has commonly been mentioned as a proof that he is of an inferior race of animals; or that he is greatly degenerated. "The beardless countenance and smooth skin of the American seem to indicate a defect of vigour, occasioned by some vice in his frame. He is destitute of one sign of manhood and strength†." From the Indian's supposed want of beard, philosophers seem to have inferred his want of strength, courage, and affection for the other sex. The Indians, like the Tartars and other Asiatics from whom they are chiefly descended, have thin beards; but writers who urge their want of beard, in proof that they are a new race of men, do not consider that there are numerous tribes or nations in the eastern parts of the old continent, who, like the Indians, appear to be without any beard. They constantly pluck it out. The islanders in the South Sea have beards, as we are told by Captain Cook; but many of them pluck it out, or the greater part of it, as well as the hair from under their arms. Whoever takes the trouble to make himself acquainted with the subject, must think it strange that an opinion destitute of truth, without other foundation than distant and hasty observation, should have obtained so general a credit in Europe. At a meeting of Indians from different tribes, in the year 1796, I examined near fifty of them, and there was not, in that number, a single Indian without a beard. There were Indians of the Chocktaw, the Chickesaw, the Cherokee, the Creek, the Chipawa, and the Shawanese nations. Their beards in general were shaved, but some of the chiefs had suffered whiskers to remain on the upper lip, or they suffered a small portion on the chin to grow to a considerable length‡. One of the Shawanese chiefs had strong whiskers upon his upper lip, and so had a Chickesaw and a

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* Robertson's History of America.   † Ibid.
‡ Lawson, speaking of the Indians on a branch of Clarendon river, in North Carolina, in the year 1700, says, "Most of those Indians wear mustachios, or whiskers, which is rare, by reason the Indians are a people that commonly pluck the hair of their faces and other parts, up by the root, and suffer none to grow."—Lawson's History of Carolina.

Cherokee

Cherokee chief. As the Indians seem to know that they have been regarded as an inferior, beardless race of men, it is not improbable that the custom of wearing whiskers, such as we have observed, by some of their chiefs, may have originated in pride ; or it may be considered as a mark of seniority and rank. A dark skin does not show the beard when shaved, but whiskers are very conspicuous. The habit of shaving is modern among the Indians, and such is probably the use of whiskers, for the ancient custom was to pluck out the beard. It was pulled out by the finger nails, as some of them allege, and others of them describe other modes by which it was extirpated. The tedious hours of an idle savage, sitting on the ground more than half his time ; without work, without books, without converse, and almost without thought, must have been relieved by the frequent and trifling exercise of plucking the beard. And it is not improbable that the desire of some employment, which required little motion, and little exertion of the mind, gave rise to that other absurd, but very common practice among savages, tatooing, or marking the skin by various paints and figures. It appears strange, at first sight, that a custom so unnatural as pricking the skin, and marking it with different paints, should prevail among the savage nations in Africa and Asia, in the South Seas and in America. The Arabs mark their lips, as well as the arms and body, with blue paint *. Customs like these, which originated in whim, or rather in the desire of relieving tedious hours by some employment, produce a considerable change in the external form: and that adventitious form is soon regarded as a criterion of beauty; it becomes general in the nation. The Indians, like the Tartars, frequently cut the hair from the greater part of their head. This custom was prior to the use of scissars among them. Some old Indians whom I consulted on this subject, allege that their ancestors, not having sharp instruments, had recourse to fire, such is their tradition, for removing the hair. They singed it off with a live coal of hickory, or some other hard wood. Those observations on the subject of beards, perfectly agree with the testimony of other people. I have been assured by traders and gentlemen who have conversed much with the Indians, and lived among them on terms of the utmost familiarity, that Indians, in all cases, have hair, exactly as white people have it; without any difference, except that it is thinner. As their taste begins to change, from their acquaintance with white people, they are less solicitous at present to extirpate those hairs which are not supposed, as formerly, to mar the beauty.

* Pietro della Valle. The savage mountaineers in the kingdom of Ava, in India, disfigure themselves by tattooing.

We

We know that women among the American Indians, are forced to perform all the hard labour that is necessary to the support of a family. The husband smokes his pipe, or sleeps in his cabin, while his wife hoes the corn, with a child at her back. By this mark of apathy, or unkindness to his female, the American savage is supposed to be distinguished from other men. " Marriage itself, instead of being an union of affection and interest between equals, becomes among them the unnatural conjunction of a master with his slave*." The author of this remark was not unacquainted with the manners of rude nations in the old continent ; and if he had sought for a satisfactory proof, that men are all of the same family, and that the disposition is not changed by an extraordinary change of climate, he would have found such a proof in the conduct of the American savage to his female.

There is no living creature on the old continent, bird or beast, that is so much distinguished as man, in his uncultivated state, by the want of kindness to his female companion. The male bird is most assiduous in helping his mate to feed their young. Some males among the beasts, when their assistance is not wanted, neglect their female; but none of them adds to her trouble, or treats her with cruelty. Man alone is distinguished by the want of kindness, and by cruelty to his female. Perhaps Russia may be the only country in which the tyranny of a husband is reduced to a system, and avowed in the marriage ceremony; but Russia is not the only part of the old continent in which the wife is a slave to her husband. The Arabian does not suffer his wife to eat with him ; he would, as he conceives, be degraded by her company; but he compels her to bring wood and water, to dress his victuals, and to perform every other menial service. His sons are taught to despise their mother. She is not suffered to eat with them after they are eight or ten years old. In that ancient nation, we see the character of men, who are not perfectly civilized, as it may be traced through all shades and colours, in the old continent, or the islands connected with it. In many of the nations in Europe, who presume to call other men savages, the weak and humble wife continues to suffer under the chastisement of a master. We have reason to believe that man is the greatest tyrant upon earth. His strength is the measure of his conduct. The little despot in his family, and the great despot on his throne, exhibit the same character. Those who are weak, may expect to smart beneath the arm of power. Women are indebted to civilization alone for the happiness they enjoy, in some parts of the world. And their situation, in every

part,*

part, must be improved by the progress of knowledge. We soon discover that all permanent happiness depends on sentiment and reflection. The consciousness of giving protection and comfort to those who place themselves under our care ; to those who are weaker and need our assistance, is the solace and reward of men who feel and reason; it is the source of their greatest happiness. The pleasure that arises from domestic attachments, from the constant exercise of kindness to a wife and children, cannot be equalled by all the other enjoyments in life. The greater part of our species, in the old world, have not discovered this truth. Idle and indolent, governed by passion and not by reason, they remain inexorable tyrants. If a separate race of men had been formed for America, in which animals are said to be less fierce, or less savage, it is probable that the man of America would have been less cruel to his female than the tyrant of the old world ; but his manners, on this head, give an additional proof that he is of the old family.

After stating the great resemblance that is found between the American savage and his savage brother in other parts of the world, it can hardly be necessary to give many other proofs that they are too much alike. The American Indians are described as men who are passionately fond of strong drink. On this head they perfectly resemble the savage and half savage of the old continent. The Tartar gets drunk with fermented mare's milk : the Mahometan, with opium and the smoke of tobacco : the ancient Scythians intoxicated themselves with the fumes of hempseed : the Celtic and Teutonic nations, with ale and mead : the African gets drunk with brandy. We say nothing of the modern nations that are more civilized, who, to the reproach of rationality, seem to have a pleasure in resembling beasts. Weary of decent deportment, and fatigued with the trouble of thinking, they deliberately sit down to deprive themselves of reason. The American savage is equally attached to drinking and gambling with his European brother.

The nations of America have been represented as men of little strength ; but as they are known to be at least equal in size to those of the other continent, they may also be presumed to be their equals in strength when they are fed in the same manner, and equally accustomed to labour. Such of them as have been employed, from Nantucket, in whaling, can hardly be matched at an oar. Activity, combined with strength, renders them excellent seamen.

The courage of the American savage has been mentioned, like his other qualities, in terms of reproach ; he is said to be " plus craintif et plus lâche *," more timid and more cowardly.

Buffon

The Indians make war by stratagem, but they are not therefore to be deemed cowards. They are not very numerous, for which reason they are not prodigal of life. The point of honour with an Indian chief, does not consist in facing his enemy in the field, but in saving his own men. Such is the dictate of prudence. The Spartan youth were trained to all kinds of stealth and stratagem, that they might the better be enabled to surprise an enemy; but the Spartans were among the bravest of men. When it became proper or necessary to face an enemy, they never turned their backs. It is admitted that Indians have shown the most astonishing degree of fortitude in bearing torture. This has been called passive courage; but the same men are supposed to be deficient in active courage; and this strange conjecture is founded on their art of war, which differs from that of Europeans. The Indian secures himself in battle by a tree or some other cover. If a cover be a mark of cowardice, our ancestors, who fought in armour, were deficient in active courage, and so are the moderns, who avail themselves of trenches or any other species of fortification. The object of an Indian chief is to destroy his enemy, with as little loss to himself as possible. Having this object in view, he avails himself of the best means in his power; nor is he afraid of reproach, while he adheres to his purpose. We have seen instances, too many, of brainless white commanders, who have sacrificed half of their men in fruitless and hopeless actions, only because they feared lest they should be suspected of the want of courage. The virtues of Fabius were not less admired, when he patiently endured the insults of an enemy, than when he met that enemy in the field. Men are less afraid of reproach, when they are conscious of not deserving it. In whatever manner the Indians may think fit to meet an enemy, they give unquestionable proofs that they are not afraid of death. Surrounded in a block-house, without ammunition, we have known them perish in the flames, because they would not surrender and become prisoners. When I say that the Indian mode of fighting, under cover, is the dictate of policy, not of fear, I am prepared to give instances, not a few, in which they have shown proofs of undaunted courage in the open field, when the other mode of fighting could not be adopted. It is found that our woodsmen are rather an overmatch for the Indians, in correct shooting with a rifle; but our chief advantage, in disputes with the native savage, must ever consist in superior numbers, or the use of cavalry. When America was first discovered, the natives appeared contemptible and dastardly, from their want of arms. A white man to an Indian was then a giant to a pygmy; but an Indian, well provided with arms, is now become a dangerous enemy.

*By

By a general view of the human race and its varieties on the old continent, and by comparing those people with the original inhabitants of America, we must be convinced that men are all descended from the same stock, and that America was peopled from the other continent; but we have no information concerning the time in which the first colonists were transported. The great extent of population in America, when Columbus made his discoveries, about three hundred years ago, is a sufficient proof that many years had elapsed since the aborigines had come to this continent; but the modern date of the largest and most populous empires then existing in America, has been supposed to justify a belief, that the first settlement of America was recent, when compared with that of the other continent. At the period to which I refer, America was settled in all directions, from north to south, although no part of it was fully peopled; nor had any progress been made in those arts which are the fruit of necessity in old and numerous societies. Those circumstances, however, can neither be urged in proof of a very ancient nor a very recent settlement. Migrations, in the old continent, have lately been the effect of a crowded population; but migrations in America sprang from a different cause. The first adventurers, who were little attached to their native soil, could hardly be attached to a particular part of the land they had discovered. Sustaining themselves without labour, in a country that abounded in game, they acquired habits of idleness. When the game became scarce in one part, they removed to another. The same spirit produces the same effects, among the present white inhabitants of North America. The more adventurous, more fickle, or more indolent, move to the frontiers, and settle upon new lands. When the range is impaired, or the game diminished, those very men, or their children, move onward, and follow the range; for they raise little corn, eating flesh instead of bread; whence their habits of idleness become inveterate. As the ocean yields a supply of food, that is more easily caught than birds or beasts, it follows that the sea coast was first explored; but the greater number of inhabitants were found in warm or temperate climates; because in such climates the means of subsistence were easy. In this manner every part of America may have been visited, and sparse settlements formed, within a few centuries after it was first discovered. In this manner too, as we are taught by civil history, the other continent was originally settled. The first migrations were not the effect of a crowded population; they were caused by a rambling or adventurous temper. Every country was first visited by single families, or by small parties, who migrated in the spirit of ambition, discontent or caprice, from young colonies or new governments. We have the names of men on
the

the other continent, who were celebrated as the founders of government; but those men, in every case, appear to have found a weak, unconnected race of savages, scattered over the country in which they fixed their empire. They certainly did not migrate from a crowded hive, whoever they may have been. When the small tribes, who first settled in America, had destroyed the game in one place, they removed to another without difficulty or opposition; but in the process of time, migrations were not effected without trouble, for all the country was claimed as hunting ground by one tribe or another. In that case hostilities commenced, and men were destroyed, that bears and buffaloes might have room to breed. The failure of game caused the Indians, in some cases, to turn their attention to agriculture: and it appears that successful chiefs, in the usual spirit of domination, in some cases extended their authority, by adding weaker tribes to their respective empires. In this manner, the monarchs of Peru and Mexico were extending their domains when the Spaniards visited this continent; and in this manner the greatest empires formerly sprang up in the other hemisphere. But Mexico and Peru may have been well peopled, many a century before there was a monarch in either of those countries.

It has been observed that the American savage, passing over the shepherd state, was turning his attention, in some instances, to the cultivation of the soil. From this circumstance it has been alleged that he differs greatly from the man of the other continent; but this inference is not correctly deduced, for it is known that the introduction of new arts and customs is frequently to be ascribed to what is called pure accident. The casual discovery of gunpowder in Europe gave rise to a variety of new customs and to the neglect of old ones. The introduction or discovery of a grain, that was easily cultivated, may have promoted agriculture; or the want of the most useful domestic animals may have caused the employment of a shepherd to be forgotten. The use of cows, sheep, and goats was known to the first family upon the other continent; and that family was also instructed in the art of cultivating the earth. The first emigrants from the original stock were equally instructed in the several arts of tilling the earth, tending cattle, and killing game; but as men always prefer the most easy mode of living, they would support themselves, for many years, by hunting alone; for in new countries, where there is any winter, a family is most easily supported by hunting and fishing. When the game failed, they had recourse, in every case, to the other most easy mode of living, to the care of cattle; for the colonists, who were never separated from the parent state by an ocean, could easily obtain a supply of cattle when they needed them. In the progress of population,

tion, when pasture failed for cattle, they had recourse to agriculture. Thus it was that the shepherd state commonly succeeded the chase, and that again was succeeded by agriculture. This succession did not, for it could not, take place in America. The first planters brought with them the usual stock of knowledge, but they brought no cattle. They brought the maize*, as I presume, that we call Indian corn, for it is said to grow in Asia. If they wished to raise cattle, they could not obtain the species to which they had been accustomed, but they could raise corn, for they had the seed : hence it was, that in all cases some degree of agriculture immediately followed the habit of living by the chase.

The annals of the American savage, like those of every other nation, have been corroded by the rust of time. When we speak of the epoch in which they arrived, we find ourselves travelling in the regions of conjecture, having few marks, and those very obscure, to direct our course. We discover nothing that may be deemed certain, except that they came, the greater part of them, from Asia, and that the time of their arrival is very distant.

While it was presumed that America was separated from the other continent by an ocean of considerable extent, various opinions were formed respecting the manner in which this continent had been peopled ; for the ancestor of an American savage, in his canoe, could not be supposed to have adventured far upon the ocean ; but the discoveries of late navigators have removed all difficulties on that head. We learn from Captain Cook and

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* Although maize and tobacco are both commonly supposed to have originated in America, there is much reason to believe that both those plants were carried from Asia by the original emigrants. I suspect that the Esquimaux Indians, when first discovered, had not the use of maize ; for their ancestors came from a part of the other continent in which that grain is not cultivated, but it is cultivated in Asia. "The inhabitants," says Labillardiere, "sold us ears of maize, still green, which had been boiled." This was in one of the Molucca islands. Tobacco, as we are told, is cultivated by the natives in the vicinity of Nootka Sound. But tobacco is a tropical plant. The seed must have been imported from Asia. The Chinese, who seldom change their habits, have long been smokers of tobacco. Certain nations in India beyond the Ganges, are slaves to the use of this nauseous plant. The inhabitants of the island Sagaleen, about the 49th degree of latitude, are also perpetual smokers of tobacco. We are told by La Perouse, that "they have good large-leafed tobacco, and the pipe is never out of their mouths." They are supposed to purchase their tobacco from the Tartars. It has also been observed, that the Tartars on the continent, nearly opposite to that island, are enslaved by the use of tobacco. "Every male of them, young and old, wears a leathern girdle, to which are hung a little pouch for tobacco, and a pipe." It is not to be supposed that all those nations, so distant and lately discovered, imported their tobacco, or its seed, within the last three hundred years, from America.

others,

others, that Asia is not far distant from America. They may
be seen, at the same time, from a ship in the middle passage *.
It has also been discovered, that all the little islands between
the northern parts of Asia and America are inhabited by savages,
who must have wandered from Asia ; and it is not to be supposed
that a similar race of men did not travel to America. In a word,
the descent of the North-American Indians, or the greater num-
ber of them, from Asiatic Tartars, or their progenitors, is now
so fully established, that I shall not exercise the reader's pa-
tience, in showing how much they resemble one another in their
features, their scantiness of beard, or their language : but the
Tartar did not transport his horse, and the want of that animal
has caused many shades of difference in their habits.

In stating that the aborigines of North-America are chiefly
descended from the Tartars, or from the same stock with the
Tartars, I am supported by common tradition † among those
people, as well as by the obvious facility of the passage. But
some of the northern Indians, as I suspect, emigrated from
Europe. It can hardly be questioned that the Esquimaux In-
dians are the diminutive sprouts of Norwegian ancestors. It is
fully ascertained, that colonies from Norway settled in Iceland
and Greenland near one thousand years ago ; but the first ad-
venturers who are mentioned in history, found a race of savages
who had preceded them.

The same adventurers who discovered Iceland, at the period
to which I refer, extended their travels to the Labrador coast,
where they found a race of savages, who appeared also, by their
language, to have emigrated from Denmark or Norway. When
we consider the distance of Iceland, Greenland, and the Labra-
dor coast, from the British isles, or the northern parts of the
continent, the difficulty and danger of navigating the northern
ocean, in high latitudes, and the contemptible vessels now in use
among the Esquimaux, it may appear strange that every island,
and every spot of land in those inhospitable regions, should have
been discovered and settled at the time to which I have referred.
This phænomenon is best accounted for by recollecting that there
must have been a time in which the northern ocean was navi-
gated with less danger than at present ; when Iceland, Green-
land, and the Labrador coast were much more hospitable, the
soil more productive, and the climate more temperate than they
are at present. This allegation may appear somewhat paradoxi-
cal, when it is compared with a former observation, that the
winter's cold has been gradually decreasing for more than 2000

* The distance is not above thirteen leagues.
† The Indians in general in this part of America allege, that they came
from the north-westward.

years,

years, in the greater part of the world. The fact, however, is not to be disputed. The natives of Labrador, from their want of letters, can give us no account of the change that has taken place in that country; but the case is very different in Iceland, The inhabitants of that island have preserved their history for nine or ten centuries, and the change of climate there has been fully established. I do not say that the numerous population of Iceland, near one thousand years ago, or the flourishing state of arts and sciences among those people, at so distant a period, is to be taken for a proof that the climate was formerly more temperate, and the soil more productive than at present, although they add great probability to this opinion; but we find an argument in the natural history of the island, that seems to be absolutely conclusive. It is not to be disputed, that in former ages Iceland produced timber in abundance [x]. Large trees are occasionally found there, in the marshes or valleys, that have been covered to a considerable depth in the ground. Segments of those fossil trees have lately been exported from the island, in proof of the fact alleged. But we are equally certain, that in the present age timber does not thrive in the island. Its growth is prevented, or the plants are destroyed by the intensity of the winter's cold, as in the northern extremities of Asia and America, where nothing but shrubs is to be found. The same pejoration of climate, and a similar degeneracy in the productions of the soil, have certainly taken place on the Labrador coast that have been observed in Iceland.

This remarkable increase of cold in high northern latitudes may be accounted for by reference to a general deluge, the flood of Noah. I am aware that allegations, in natural or civil history, are not to be supported by referring to a book whose authority is disputed; but in the present case I must be permitted to allege the certainty of a general deluge, provided it will account for the several phænomena, and provided those phænomena cannot otherwise be accounted for.

Upon the supposition of a general deluge, it will be admitted that immediately after the flood there could be no ice in any part of the ocean. The waters in the northern regions were exactly of the same temperature as the waters in other parts of the ocean, for they had the same origin. The fountains of the great deep

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* It is asserted in the ancient Icelandic records, that when Ingulf, the Norwegian, first landed in Iceland, anno 879, he found so thick a growth of birch-trees, that he penetrated them with difficulty. Some modern historians, knowing that no trees of any kind grow at present in that island, have expressed their fears lest the veracity of the ancient annalists should be suspected. If they had known that trunks of trees have lately been found in that island, buried several feet deep in the earth, their fears would have been obviated.

were

were broken up. The temperature was thirty or forty degrees above the freezing point. In that case, the air in Iceland, or upon the Labrador coast, coming from the temperate surface of the ocean, was temperate and pleasant. Vegetation in the long days of summer was vigorous, and the winter was not sufficiently cold to destroy perennial plants. In the process of time the waters near the pole lost a great part of their heat, and ice was formed in the creeks and bays. Large cakes of ice were occasionally broken off by storms, and detached from the shore. As the temperature of the ocean decreased, some part of the broken ice continued to float through the summer. Every succeeding year added to the size of the floating masses*. They were increased by rain, by snow, and by the spray of the sea. The northern ocean is nearly filled at present by those floating islands of durable ice. The summer winds that reach the coast, instead of being tempered, as formerly, by a watery surface, are now chilled by mountains of ice ; and they are become so intensely cold in winter, as to be destructive of animal and vegetable life.

It may possibly be alleged, that in the space of three thousand years, the time that passed between the flood and Ingulf's arrival in Iceland, the atmosphere should have been as cold, and the accumulation of ice as great in the northern ocean as they are at present. It is readily admitted, that when we consider the present degree of cold which prevails in high latitudes, we conclude that a few years would be sufficient to produce vast bodies of ice. But we are to consider, that in the case referred to, the water in every part of the ocean was tepid, and the whole face of the earth was of the same temperature with the water ; whence it followed, that the atmosphere could not be cold, nor could there be ice or snow in the longest nights of winter. We have no data by which we may compute the number of years or ages that were necessary to abstract so great a body of heat as then existed in the northern lands and ocean; but a long period must have been required, for there is no fact in natural history more certain than that there was more heat, or less cold, in high northern latitudes, in the eighth or ninth century, than there is

* It is a curious fact, and in perfect coincidence with this theory, that when the first Norwegian colony settled in Greenland, about one thousand years ago, they found no difficulty in approaching the coast, and a regular correspondence was supported with those people for many years. That intercourse was entirely neglected during the dark ages of anarchy and misrule in Europe. Since the revival of learning, within the two last centuries, sundry attempts have been made to discover the remains of that colony, who lived on the eastern part of Greenland: but no landing can now be effected on that coast, by reason of the vast bodies of ice with which it is pressed. From this it is clear, that within the last seven or eight hundred years there has been a great increase of ice in high northern latitudes.

at

at present; nor is it clear that the heat of the air, earth, or water, in those high latitudes, has yet attained its lowest degree *.

By keeping in mind that there has been a time in which the climate was temperate, and the soil, for the same reason, was productive in high northern latitudes, we are enabled to account for many phænomena which had appeared very enigmatical. We are no longer surprised that any part of our species should have migrated and settled themselves willingly in Lapland and other regions near the arctic circle; in regions from which nature, in the present age, seems to shrink with horror. Those countries, as we conceive, were all of them settled while the climate was temperate and the soil fit for cultivation. As the miseries that are caused by cold weather and a frozen soil increased, the habits and constitutions of the inhabitants suffered a considerable change, and they became attached to the land of their ancestors. They now live, or seem to live, contented, in a country to which criminals are banished as one of the severest punishments.

By attending to the above-stated changes in soil and climate in high northern latitudes, we can easily discover how it should have happened that Norway contained a crowded population above one thousand years ago, and sent out colonies.

By attending to that change of climate in high latitudes, we can easily account for incidents that have excited general attention twelve or fifteen hundred years ago. We discover how it happened that certain countries, which at present are not very desirable nor productive, had formerly been the *officina gentium*, the very nursery of nations ; and why, in the process of time, it became necessary for those very people to migrate by thousands in quest of better habitations.

XLV. *On a new Species of Calculus.*   *By Mr.* J. T. Cooper.

To Mr. Tilloch.

Sir,— In July last I sent you an account of a new species of calculus, page 27 ; and having submitted it to a more correct analysis, I now transmit you the result.

Ten grains of it, after being heated to about 220° Fahrenheit, for the space of twenty minutes, gave of

Carbonate of lime	..	9·3
Phosphate of lime	...	·4
Oxide of iron	..	·2
Loss ..	..	·1
		10·0

* Vast bodies of ice from the northern seas are thought to have become more dangerous of late to navigators, near the banks of Newfoundland, than formerly.

I was

I was not able to find the least trace of uric acid.

Finding the substance to consist almost wholly of carbonate of lime, I suggested the probability there might be of dissolving the calculus in the bladder by the action of dilute muriatic acid. Accordingly this was had recourse to; and I am happy to add, that by the injection of two drachms of muriatic acid diluted with eight ounces of water, heated to about 100°, every day, the whole of it has been removed, and that in eleven days. The acid during almost the whole period came away nearly neutral. I think it not improbable that in time a solvent for urinary calculi of every description may be discovered, and render cutting for the stone, that most dreadful of all operations, perfectly unnecessary.

<div style="text-align:center">I remain, sir,

Yours, &c. &c.</div>

76, Drury-Lane, Sept. 14, 1816.   JOHN THOMAS COOPER.

XLVI. *Extract from a Memoir read to the Institute on the 13th of May* 1816, *on the Possibility of making the Molluscæ of Fresh Water live in Salt Water, and vice versâ. By* F. S. BEUDANT[*].

THE experiments which form the object of this memoir have been undertaken chiefly with a view to account for some remarkable geological circumstances, and particularly for the mixture of sea- and fresh-water shells in the same rock, which M. Beudant discovered in 1808, at Beauchamp near Pierrelaye, in the department of the Seine and Oise, and more recently in the valley of Vaucluse.

<div style="text-align:center">*Experiments on Fresh-water Molluscæ.*</div>

Towards the end of summer 1808, M. Beudant first attempted to convey suddenly fresh-water molluscæ into water charged with 0·04 parts of muriate of soda, as sea-water generally is. These animals could not withstand the change; for their shells contracted, and they died in a few seconds. The bivalve molluscæ merely resisted the salt-water a little longer, in consequence of shutting their shells: in a few days, however, they died also.

Being desirous of accounting for the almost total absence of fossil shells in the beds of gypsum, M. Beudant tried the action of water charged with sulphate of lime on the molluscæ; and with this view he plunged some lymnæi suddenly into this mix-

<div style="text-align:center">[*] *Annales de Chimie et Physique*, tome ii. p. 34.</div>

<div style="text-align:right">ture.</div>

ture.   They did not appear at first much affected, but in eight days they were all dead.

As we do not know positively, as yet, if the shells which are found in certain beds of saccaroidal limestone of transition, and in certain compact secondary limestones, have lived in the liquid itself, or have formed those soils themselves, or, finally, have been brought there accidentally; as, on the other hand, it is very probable that these kinds of chemical deposits have taken place only by the intermedium of a liquid, the solvent faculty of which was owing to an excess of carbonic acid,—M. Beudant tried the action of water charged with this acid on the molluscæ. With this view he employed Seltzer water.—The animals plunged into this water were quickly suffocated.

Water charged with other mineral acids in very small quantities produced nearly the same effect.

Water charged with 0·02 of sulphate of iron affected in the same way the molluscæ plunged into it;—in a few seconds all the species submitted to this test, whether univalves or bivalves, died.

Water saturated with sulphuretted hydrogen, and afterwards diluted with its weight of clear water, produced much slower effects : several molluscæ even made various movements in it ; but in a few days they died, the one after the other.

After these preliminary trials, it was natural to examine if it was not possible gradually to habituate the molluscæ of fresh water to live in the waters into which they had been previously plunged so suddenly. In spring 1809, therefore, M. Beudant collected a great number of fresh-water molluscæ in the environs of Paris : he separated the species, and divided the number of individuals of each of them into two equal portions, putting each into a vessel by itself : he thus formed two series of the same species, each including the same number of individuals : he preserved one as a point of comparison in Seine water, and upon the other he attempted the gradual transition which he had in view.

M. Beudant had at first employed for several days water containing only one grain of salt per pound, *i. e.* about 0·00011 : he afterwards successively increased the quantity of salt; first by adding a grain every two days, and then every day; and latterly, three grains a day. The experiment lasted six months ; at the end of which time the liquid employed contained 0·04 of its weight of salt, comprising about 0 005 of muriate of lime.

By proceeding in this way, most of our fresh-water molluscæ were completely habituated to live in salt water, in which they seemed to be perfectly at ease. But in order to make the experiment precise, and to draw certain consequences from it,

          M. Beu-

M. Beudant noted carefully the number of individuals which died, on the one hand, in fresh water; and on the other, in salt water: it was in this way that he ascertained the differences about to be noticed.

All the species of lymnææ and planorbes, the *physis* of cold springs; and the *patella lacustris* live perfectly well in salt water. In fact, in the space of five months, there died in the fresh water fifty-four of the hundred animals which had been kept in it, and in the salt water there died fifty-seven of the hundred; the difference is scarcely worth mentioning.

On the contrary, the other species of paludines (*helix vivipara, helix tentaculata* of Linnæus), and the nerites of the Seine seemed to suffer from their long continuance in salt water, and more died than in fresh water. In short, in the fresh water only forty of the hundred died; and in the salt water seventy-one of the hundred died.

All the bivalve molluscæ, the anodontes, the mulettes, and the cyclades, put into salt water died during the experiment, and before the liquid had attained the degree of saltness which it was necessary to attain. On the contrary, they lived the whole summer in fresh water, and continued in it at the end of autumn. It must be remarked, however, as an important result, that these same species lived perfectly well in water charged with only 0·02 of salt.

M. Beudant also resumed the experiment which he had begun with water charged with sulphate of lime, in order to habituate the molluscæ gradually to live in it. At first he diluted with a great quantity of Seine water, the water which was strongly saturated with this kind of salt: afterwards he kept diluting it gradually; and lastly, he employed it completely saturated. The molluscæ at first lived very well: subsequently several died, in proportion as the quantity of salt was increased; and lastly, the few remaining died when the saturated liquid was used. This experiment repeated at two subsequent periods gave the same result.

We now come to the experiments on marine molluscæ which M. Beudant made in 1812 and 1813.

He collected several individuals of various kinds of marine testaceæ, such as the patellæ, fissarellæ, crepidulæ, haliotideæ, sabots, cerites, buccinæ, tellines, venuses, oysters, &c. &c.

Several individuals of the above genera, when plunged suddenly in fresh water, contracted within their shells, and died in that state. Those species only which live habitually on rocks placed out of the water lived a little longer, but they soon perished also.

M. Beudant, in order to proceed afterwards to the experiment

proper

proper for determining the gradual transition of the sea molluscæ in fresh water, also formed two series of the same species. He preserved one constantly in sea water, and employed for the other sea water diluted successively with fresh water. Five months afterwards he employed nothing but fresh water.

By means of this precaution, the marine molluscæ were habituated to fresh water, in which they lived during five months more with lymnææ and planorbes. There were some peculiar circumstances attending them, which M. Beudant relates; viz.

The patellæ of the vulgar species, and the *sabots*, which are both sea shore shellfish—the cerites, the columbellæ, the venuses, the oysters, &c. perfectly resisted the experiment. There died thirty-four out of the hundred individuals constantly kept in sea water, and thirty-six out of the hundred of those exposed to fresh water.

But the dragon-headed patellæ, the fissurellæ, the crepidulæ, the undated buccinæ, the cameos, &c. all died during the experiment. It ought to be remarked, however, that all the species lived perfectly well in sea water diluted with an equal portion of fresh water, *i. e.* in water containing about 0·02 of its weight of salt.

Having learned by Lavoisier's analysis that the waters of lake Asphaltus contained 0·40 of saline substances[*], and as according to travellers no organized living body is to be found in it, M. Beudant was desirous of knowing what degree of saltness marine animals could support. Consequently he added impure muriate of soda to common sea water, and he was convinced that all the sea molluscæ which he had at his disposal lived, without any apparent inconvenience, in water containing 0·31 of saline matter composed chiefly of muriate of soda, with some centimes of muriate of lime and muriate of magnesia. But as often as he increased the quantity of salt, so as that small crystals were formed on a slight evaporation in the air, the animals shrunk within their and died.

M. Beudant concluded by drawing the following inferences:

1. Since there is reason to believe that the same water, whether fresh, or salt like that of the sea, or even salter, is capable of supporting all the animals which frequent marshes, rivers, and the sea-shore, it may be presumed that similar circumstances have existed in nature, and to these circumstances it is owing that we see in one and the same stratum sea and

[*] Lavoisier's analysis gave

Water	-	-	-	-	-	55·60
Muriate of soda	-	-	-	-	6·25
Muriate of lime and muriate of magnesia	38·15

100·00

land

land shells; admitting always, as every thing seems to prove, that these shells are still in the place where they have lived originally.

2. If we could suppose with some naturalists, contrary to all appearance, that the strata called fresh-water strata have been all formed under sea water, the present experiments will account for the absence (in other respects very remarkable) of the bivalve river shells of the genera anodontes, mulettes, and cyclades. In fact, we have seen that the molluscæ which live in these shells were not able to live in water charged like that of our seas, with 0·04 of saline substances.

3. Since it results from experiments many times repeated, that the molluscæ, at least those of fresh water, cannot live in water charged with sulphate of lime, we can account for there being no shells in the gypseous mass of Montmartre, and in general in the old or new gypsums, although they are frequently in layers subordinate to shelly strata.

4. Since the marine molluscæ can live in water almost saturated with muriate of soda, it would seem that the absence of living organized bodies in lake Asphaltus, if this really be the case, is owing to the presence of the bitter muriates of lime and magnesia, and perhaps to that of the bituminous matters which, Lavoisier has not found in his analysis, without doubt, because they are but occasionally met with and under certain circumstances only.

On the other hand, since the sea molluscæ perish in water hypersaturated with muriate of soda, it is not astonishing that none of their remains have been found in the immense masses of sal gemma, which are found in various countries.

5. Lastly, if we admit that sea and fresh water molluscæ can live in the same liquid, it would seem to result, that the fact of living in fresh or in salt water cannot be a reason for establishing particular genera, unless we can find sufficient and constant characters in shells, or, what is better still in the animals which occupy them, when they are not fossil.

XLVII. *On the Precipitation of the Oxide of Gold by Potash. By* M. FIGUIER. *Extracted by* M. GAY-LUSSAC[*].

GOLD, the properties of which have been so long too imperfectly known, and on which much yet remains to be done to place its history on a level with that of most of the other metals, has been recently diligently examined by M. Figuier, an eminent chemist of Montpelier. Messrs. Vauquelin, Duportal and Pelletier had

asserted

asserted that the alkalis did not precipitate the solution of gold in the cold way. M. Oberkampf had obtained a contrary result; but he thought he could account for this difference by the excess of acid of the solution, which gave rise, with the precipitating alkali, to the formation of a great quantity of triple salt, indecomposable, at least totally, by an excess of base; for it is certain that there is no precipitate made in the cold way in the space of several minutes, even with ammonia, when the excess of acid is sufficient. The object of M. Figuier is to prove that the excess of acid of the solution of gold does not prevent the precipitation of the oxide by an alkali, and that we obtain nearly as much of it as from a solution as neutral as possible.

"Six grammes of dry muriate of gold," says M. Figuier, "were dissolved in 150 grammes of distilled water; the solution when filtered was divided into two equal parts, and put into two conical glasses. Into one I poured four grammes of muriatic acid: I saturated both solutions with caustic potash dissolved in water: their yellow colour became a deep red. A short time afterwards, the liquors became turbid: a flaky precipitate of a gray colour was developed: these precipitates increased after some time and became deeper:—after forty-eight hours they appeared to be at their maximum of augmentation and colour. I observed no difference between the two, only that in order to produce it, we must employ a greater portion of potash in the acid muriate of gold, than in that which is less so; but I constantly observed that in both cases it was requisite that the potash should be in excess, in order to make the precipitates abundant. After having filtered these liquors separately, I put them into small retorts and exposed them to the action of a moderate heat; new precipitates were still formed much deeper in colour. I added them to the first, washed them with pure water, and dried them. When weighed in this state their weight represented two-thirds of the gold which was in solution;—that obtained from the acid solution did not differ from that which I by a decigramme of acid. The liquors which had furnish precipitates still contained gold: they became yellow by the fusion of the muriatic acid, and gave new precipitates by potash, although there was an excess of acid before the addition of the alkali, so that by treating them alternately by potash and muriatic acid, I succeeded in extracting the whole gold contained in the muriate, as I have stated in a former paper.

Note by M. Gay-Lussac.—If we can separate the whole gold of the solution by means of the alkalis, it is undeniable that the triple salts are entirely decomposed by an excess of base: nevertheless, if the observation of M. Figuier be exact, that in

order

order to precipitate the whole gold from a solution into which an excess of alkali has been put, we must add acid and super-saturate again with alkali, we may easily explain by this circum-stance the results of Messrs. Vauquelin and Oberkampf, for they did not think of the process employed by M. Figuier. In short, we have such a confused idea of this process, that although we do not doubt its efficacy, we should like to see it repeated by other chemists.

XLVIII. *Notices respecting New Books.*

Col. James Capper has lately published, at Cardiff, in South Wales, a work of considerable merit, entitled " Meteorological and Miscellaneous Tracts, applicable to Navigation, Gardening, and Farming, with Calendars of Flora for Greece, France, Eng-land, and Sweden." The following subjects constitute the most interesting part of the work:

Observations on the cause and consequences of the tempe-rature in different latitudes; and observations on the aurora borealis, and the frequent occurrence of tempests about twenty-four hours after its appearance. There are also some curious remarks on the lunar influence on terrestrial bodies, and some very useful and interesting observations on the local varieties of temperature in different parts of Great Britain.

According to the German papers, a clergyman of Iceland, named Johnson, has recently translated into Islandic verse The Paradise Lost of Milton.

A gentleman at Cardiff is about to publish A Translation in the Welch Language of Mr. Arthur Young's Farmers' Calendar.

Mr. Forster has just published a work entitled " Flora Ton-brigiensis ; or, A Catalogue of Plants growing in the Neighbour-hood of Tonbridge Wells.

Messrs. Netlam and Francis Giles, of New Inn, London, are now making arrangements for A Trigonometrical Survey (founded on the basis of Col. Mudge's and Capt. Coleby's Triangles) for A New Map of the County Palatine of Lancaster, on a scale of one inch to the mile ; to be dedicated by permission to His Royal Highness the Prince Regent : and to be published by subscription, at four guineas in sheets, or five guineas on can-vass and roller, or half-bound in atlas.

The survey is commenced, and will be carried on with dili-gence.

Captain Lewis Granholm, of the Royal Swedish navy, \as in the press a translation from the Swedish of "Experiments on the resistance which bodies experience when propelled in a straight line through water." These experiments were made by Admiral de Chapman, a distinguished naval officer, and superintendant of the dock-yards of Sweden : they exhibit a chain of research which has led to the valuable and remarkable discovery of a law of nature regarding water, which could not possibly have been attained by mathematical induction, or any reasoning *a priori*, and which involves conclusions of the greatest importance, not merely as to the *best form* for the construction of ships capable of moving with the greatest velocity through water, but even as respects the best form for piers of bridges in rivers subject to inundation, or where the current is rapid. The work itself is translated from one of the few copies printed for the use of the author's friends ; and the Lords of the British Admiralty have given their high sanction to the publication in an English dress.

Mr. Dyer's work on the Privileges of the University of Cambridge, having much exceeded his original intention, will not appear till winter, and instead of one will occupy two large volumes 8vo. Besides the Chronological Tables and Charters of the University, and corrections of his History, as announced in his Proposals, these volumes will contain in English (some part of the work being in Latin) Memorials of the Rise and Progress of Printing at Cambridge, with an account in succession of the Printers, and the principal books published by them ; the connexion of its present with its former state of literature, and an attempt to bring the literary biography of the University down to more modern times.

XLIX. *Intelligence and Miscellaneous Articles.*

NEW VOYAGE ROUND THE WORLD.

A FRENCH merchant of Bourdeaux has equipped a vessel for the circumnavigation of the globe. It is described as a strong swift-sailing vessel of 200 tons burden, called the Bordelais, and will be commanded by officers of the French navy. The following has been published in the French journals as the projected track of this expedition. The Bordelais will double Cape Horn, and will not anchor until she arrives off the coast of Chili : she will then proceed to California, visit Nootka Sound and the adjacent shores, trafficking with the natives for peltry ; this track will

will b, continued to Cook's river along the coast, and as far north as possible. From Nootka Sound the vessel will proceed to th: Sandwich Islands for pearls and sandal wood, &c. The circumnavigators will then proceed to China, and from thence return to Bordeaux.

FRENCH COLONIES.

The French Government have sent out an expedition to Senegal, with a view to re-colonize that once flourishing settlement. The French frigate *La Meduse*, which carried out the governor and settlers, was unfortunately wrecked, and about 150 individuals perished in a most dreadful manner from hunger, thirst, drowning, insanity, and mutual assassination, on a raft on which they were abandoned for many days to the mercy of the waves. The following account is given in the French papers of the success of the survivors:

Paris, Sept. 22.

The latest intelligence from Senegal announces, that not only the boats of the French frigate Meduse had arrived at St. Louis, but that the governor and officers had even saved all their effects. Part of the ship-wrecked mariners had followed along the coast, and arrived without any accident, except M. Kummer the naturalist, and one of the delegates of the Philanthropic Colonial Society. This traveller, having wandered away from his companions to discover fresh water, found an abundant and limpid spring; but while he was drinking, he was taken prisoner by a party of Traersas-Moors, to whom he called himself an officer of high rank, which made them conduct him to the French fort, mounted on a fine horse. The governor rewarded them handsomely. All the instruments of agriculture sent by the Colonial Society were swallowed up in the water. This is a heavy loss, and will retard all attempts at cultivation for at least a year.

The delegates who are in the village of Dacar have already explored the peninsula of Cape Verd, where they hope to form their first establishment. This peninsula is not very fertile, but it has nevertheless all the resources necessary for forming a colony: it has land capable of being ploughed, salubrious water, a temperate climate, and good pasturage: it is separated from the rest of the states of Damel, of which it forms part, by defiles, where three or four hundred inhabitants of the peninsula lately arrested the progress of the whole army of Damel, ten thousand men strong This African prince seems disposed to cede a territory in which he exercises a most uncertain authority. The delegates of the Colonial Society expect to come to an understanding with the inhabitants. One of them, M. Parsen, has drawn up a report on the subject, and transmitted it to France.

The

The French have also formed a colonial establishment a Rio Janeiro, but not with the official sanction of the Governnent. This colony is formed of male and female emigrants who vere expatriated in consequence of the late political changes in France. It consists of 400 persons: they have obtained from the Portuguese government three houses, with furniture, and some neg-es as domestics; they receive daily rations of fish, flesh, fruits, and Madeira or Port wine. Every planter receives from the State a large portion of ground; but this liberality is of little value, from its being difficult to clear in consequence of the want of instruments.

EXPEDITION TO AFRICA.

In our last number, p. 152, for " Captain Campbell has barometers (principally Mr. Arnold's)" read " has chronometers," &c.

We ought to have noticed in the same article that Major Peddie and Captain Campbell, instead of carrying out barometers as is usually done, fitted up with frames, for ascertaining the height of such mountains as they may meet with on their excursion, took with them a number of plain graduated tubes, any of which they can fill when occasion requires with mercury, and by this means obviate the inconvenience often arising from getting barometers broken on like excursions. Captain Campbell took a dozen tubes, any one of which it requires but little dexterity to fill, and which when filled and inverted in an open vessel is ready for use.

IMPROVEMENT OF RAPE OIL.

A Simple Method of rendering Rape Oil equal to Spermaceti Oil, for the purposes of Illumination *.

To the Secretary of the Cork Institution.

Sir,—Nothing struck me more, on my arrival in Ireland, than the little use made of the patent lamp, which is in such common and universal use in England and on the continent. But my surprise in a great measure ceased, on finding spermaceti oil at such an exorbitant price in Ireland.   In London it is sold by retail, in the dearest shops in Bond-street, at seven English shillings a gallon, and here, at twelve Irish shillings per gallon,—an extravagant price every one must allow, and fully sufficient to check the use of this lamp in a very great degree.

I thought I should render the public a very great service, could I, by some mode, render any of the cheap oils of our country effectual for the service of that lamp. Many persons in England told me, that our oils were of too gelatinous and viscous a nature to be drawn up in the capillary tubes of the cotton wick. I there-

* From No. XVIII. of the Munster Farmer's Magazine.

fore

fore perceived the first thing I had to effect was, to deprive them as much as possible of those two bad qualities. I entered into a variety of experiments, some of them long and tedious, which I shall not take up your time by enumerating.

I began by washing the oil with spring water ; which is effected by agitating the oil violently with a sixth part of the water. This separates the particles of the oil, and commixes those of the water intimately with them. After this operation, it looks like the yolks of eggs beat up.—In less than forty-eight hours they separate completely, the oil swimming at top, the water with all feculent and extraneous particles subsiding at the bottom. I improved very much on this, by substituting sea-water in the place of fresh water. I tried whether fresh water impregnated with salt, may not do as well as sea-water ; but found the light not so bright, and of a reddish cast.—The oil which I have washed is rape oil, for which I am charged 4s. 4d. a gallon. I have now made use of it constantly for two months : it gives no bad smell, and when burning close to the spermaceti oil is not to be distinguished.

A friend of mine has calculated the expense of a lamp against that of a pair of mould candles, and says the lamp is much cheaper. After this, is to be compared the elegance of the one against the vulgarity of the other.

Count Rumford estimated one patent lamp to give as much light as four wax-candles, from the whiteness and brilliancy of its flame.

By the process of washing, the oil does not lose one-hundredth part. The experiment can at all times be made in a glass decanter. I purpose making it in a churn, with a cock at the bottom, the water to come up very near to the cock, by which all the oil can be drawn off, after it has deposited its impurities.

I have the honour to be, sir, your very obedient servant,
Trabolgan, March 1, 1816.   EDWARD ROCHE.

STEAM ENGINES IN CORNWALL.

By Messrs. Leans' Report of work done by steam-engines in Cornwall, it appears that the average work of 20 engines reported for August was 19,908,723 pounds of water lifted one foot high with each bushel of coals consumed. And that, during the same month, Woolf's engine at Wheal Abraham (with a load of 3 lib. 1 per square inch in the cylinder) lifted 28,933,734 pounds one foot high, and his other engine at the same mine (loaded 15·1 per square inch) 49,655,962 pounds one foot high with each bushel ; and his engine at Wheal Vor (loaded 14·4 per square inch) lifted 40,098,970 pounds one foot high with each bushel of coals.

Coul, Sept. 9.

The earthquake was scarcely felt beyond lat. 58 deg. nor was the concussion at all severe southward of the Tay: on the western coast, it was distinctly felt at Gareloch : more severely at Applecross, and less severely to the southward. From the interior I have got no accounts that can be depended on. Two men, who were travelling eastward from Loch Carron, said to a friend of mine, that when they were crossing the hill which is nearly the summit level between Loch Carron and Contin, not far from a place called Carnoch, the shock threw them on their backs. If this be true, it proves that it came from the westward ; as, if it had come in the contrary direction, and met them, they would have fallen forward. Though more damage was done at Inverness than elsewhere, there is no reason for supposing that the seat of the earthquake was under that town. If we suppose that the motion was communicated circularly from a centre, and that the centre was below Inverness, we find that Aberdeen, where the shock was pretty smart, and Perth, where it was very distinct, are nearly at the same distance, viz. about 80 or 90 miles, in a straight line. Now, the shock was scarcely felt at Cromarty, and was barely perceptible at Tarbat Ness, which is about half that distance from the centre. The reason why Inverness sustained so much damage is, that the town is built mostly on gravel, and partly on mossy ground, the shifting of which might have done more damage than actually happened. In a mountainous country, the communication of motion must be so much modified by the mountain masses, that it will probably remain always uncertain, from underneath what spot the shocks of earthquakes proceed. That the late shock had its origin under some part of Inverness-shire, there is no reason for doubting. I have remarked, that notwithstanding the very unusual quantity of rain that has fallen during this season, the rivers of the north are not at all swollen. In ordinary summers, I have known them reach to the top of their banks, in consequence of a few days rain : with a greater quantity of late, that effect has not been produced. Some springs, too, I have observed to be unusually scanty ; but my observation of springs has not been very general.—From the extent over which the shock was felt, there is reason to suppose that the blow was deep seated. Hence, though we may expect other visitations of this kind, we need not fear the eruption of the internal fire in our time. From what I have observed in volcanic countries, I have no doubt that earthquakes are occasioned by the production of a vast quantity of elastic vapour, the prodigious force of which suddenly bursts its confinement. Steam is the most probable
bable

bable agent; though other fluids may also act with sufficient energy. Whether there has been any connexion between the uncommon badness of the weather, and the subterraneous phæ-nomenon which has caused so much speculation, the imperfect state of the science of meteorology prevents us from determining. We already know th⬛⬛here is a connexion between the electri-cal state of the eart⬛⬛nd that of the atmosphere; but philoso-phers have not as yet thought the investigation of this subject worth their pains. The present theory of the atmospheric phæ-nomena does not appear to me satisfactory; and I doubt not we shall have new theories very soon. The field is open and wide, and wonderful discoveries may yet be expected.

A hard body, supposed to be of the nature of a meteoric stone, fell, during a hard thunder-storm, into a window at Glastonbury about a month ago.

LECTURES.

The following arrangements have been made for *Lectures* at the Surrey Institution during the ensuing season :

1. On Chemistry, by John Murray, Esq. to commence on Tuesday, November 12th, at Seven o'clock in the Evening pre-cisely, and to be continued on each succeeding Tuesday.

2. On Aërostation, by John Sadler, Esq. to be delivered on Friday Evenings, November 15th and 22d, at the same hour.

3. On the Principles and Practical Application of Perspective, by John George Wood, Esq. to commence on Friday the 29th of November, and to be continued on each succeeding Friday at the same hour.

4. On Astronomy, by John Millington, Esq. Civil Engineer, to commence in January 1817.

5. On Music, by W. Crotch, Mus. D. Professor of Music in the University of Oxford, to commence in February 1817.

Mr. Cooper will commence his Lectures on Chemistry in his Laboratory, 76, Drury-Lane, on Tuesday the 22d of October, at Eight o'clock in the Evening precisely.

Further Particulars may be had on application as above. Those gentlemen who wish to attend the Course are requested to make known their intentions previous to the 15th of October.

Dr. Spurzheim continues to give lectures at Edinburgh to a very large medical class of professors and others, on the ana-tomy of the brain.

Mr.

Mr. Guthrie, Deputy-inspector of Hospitals, will commence a Course of Lectures on the Principles and Practice of Medical and Operative Surgery, on the first Wednesday in October, at nine o'clock in the morning, at his house, No. 2, Berkeley-street, Berkeley-square.—To be continued Mondays, Wednesdays and Fridays.

Two Courses will be delivered during the season.

The Operations in Surgery referred to in the Lectures will be shown at the York Hospital, Chelsea, on the Friday mornings, and the treatment illustrated by such cases as may be in the Hospital.

Terms of attendance five guineas, which renders the pupil perpetual.

Medical Officers of the Navy and Army will be admitted to these Lectures gratis, on obtaining a recommendation to that effect from the heads of their respective departments.

LIST OF PATENTS FOR NEW INVENTIONS.

To John Hawkins Barlow, of Leicester Place, Leicester Square, goldsmith and jeweller, for certain improvements on tea-urns, tea-pots, tea-boards, or tea-trays.—27th June 1816.—6 months.

To John Barlow, of the town of Sheffield, founder, for a new cooking apparatus.—2d July.—2 months.

To John Towers, of Little Warner Street, Cold Bath Fields, chemist, for a tincture for the cure and relief of coughs, asthmas, and diseases, which he intends to denominate "Towers's New London Tincture."—11th July.—2 months.

To Henry Warburton, of Lower Cadogan Place, Chelsea, in consequence of a communication made to him by a certain foreigner residing abroad, for a method of distilling certain animal, vegetable, and mineral substances, and of manufacturing certain of the products thereof.—27th July.—4 months.

To Robert Salmon, of Woobourn, in the county of Bedford, for further improvements in the construction of machines for making hay, which machines so improved he denominates Salmon's new patent self-adjusting and manageable hay machines.—27th July.—2 months.

To John Hague, of Great Peare-street, Spitalfields, London, for certain improvements in the method of expelling the molasses or syrup from sugars.—27th July.—6 months.

To William Henry, of Manchester, for certain improvements in the manufacturing of sulphate of magnesia, commonly called Epsom salts.—3d August.—2 months.

To John Poole, of Sheffield, for his brass and copper plating,
or

or plating iron or steel with brass or copper, both plain and ornamental, and working the same into plates, bars, and other articles.—3d August.—6 months.

To John Chalklen, of Tower-street, Seven Dials, for certain improvements in or on the valve-water closets, and on the frames or stools thereof.—3d August.—2 months.

To John Welch, of the borough of Preston, for his improvement in the manner of making rollers used in spinning of wool, cotton, silk, flax, tow, or any other fibrous substances.—3d August. —2 months.

To John Dayman, of Tiverton, Devon, for a method of covering or coating iron, steel, or any other metals or mixture of metals, with tin, lead, copper, brass, or other metals or mixture of metals.—3d August.—6 months.

Meteorological Observations kept at Walthamstow from August 15 to September 15, 1816.

[Between the Hours of Seven and Nine A.M.]

Hour. Therm. Barom. Wind.

August

15	57	29·52	S.—Sunshine; showers; bright star-light.
16	57	29·51	S.—Showery; sun and showers; cloudy and windy.—Moon, last quarter.
17	52	29·73	NW.—Gray morning; wind; some sun; showers; great shower 3 P.M.; dark and cloudy.
18	58	29 99	NW.—Clear and clouds; and wind; sun and clouds; slight showers after 4 P.M.; clear star-light.
19	55	30·10	NW.—Sun and clouds; cloudy.
20	58	30·10	NW.—Slight rain; fine day; clear star-light.
21	59	30·13	NW.—Cloudy; sunny day; clear star-light.
22	58	30·10	NW.—Slight rain; sun and clouds; star-light.
23	59	30·10	NW.—Clear and clouds; some sun; slight showers; dark and rain. New moon.
24	61	30 10	NW.—Gray morning; black clouds; *nimbus* NW.; sun; showers; dark night.
25	55	30·26	N.—Gray; hazy early; sun and clouds; star-light.
26	60	30·20	N.—Sun; fine day; star-light.
27	58	30·10	N.—Gray *cirrostratus,* and clear; wind, and *cirrocumulus*; star-light.
28	57	30·15	N.—Sun; fine day; star-light.

August

Hour. Therm. Barom. Wind.

August

29	59	30·25	N.—Foggy; sun and wind; fine day; dark night. Moon first quarter.
30	53	30·04	NW.—Clear and clouds; gray day; wind; dark night.
31	54	29·20	SE–NW–S–N.—Rain till near noon; sun and clouds; showers afterwards; wind variable all day; rainy and very windy.

September

1	49	29·49	NW.—Showers and wind; very showery; great hail and wind; stormy; showers and wind.
2	42	29·60	NW.—Clear; clouds and wind; sun and wind; *cirrostratus* NW.; star-light; cold evening.
3	49	29·80	SE.—Sun; clear; hazy; some *stratus*; clouds and wind; great shower 4½ P.M.; sun and hazy; cloudy and sun; showers again.
4	49	29·45	SE.—Faint gleams of sun; great shower; stormy; clear and clouds.
5	55	29·60	NW.—Sun; clouds and wind; clear moon and star-light.
6	45	29·90	NW–SE.—Foggy; sun and hazy; rain after 5 P.M. Full moon.
7	55	29·90	S.—Cloudy; fine hot sunny day; clear; clouds and moon.
8	56	29·90	NW.—Clear and clouds; sun; windy; fine day; moon and star-light.
9	55	29·84	S.—Rain; rainy day; *cirrostratus* or wane-cloud; rainy; stormy night.
10	57	29·50	S.—Wind and sun; sun; clouds and wind; 9 P.M. double moon; clear moon and star-light.
11	·50	29·90	S.—Sun and hazy; cloudy; sunshine; clear star-light and moon-light.
12	52	30·00	NW.—Sun; clouds; fine day; moon and star-light.
13	55	30·21	SE.—Sun and hazy; cloudy; slight rain.
14	62	30·10	SE.—Clouds and sun; fine hot day; clear star-light. Moon last quarter.
15	59	30·10	SE.—Hot sun; fine hot day; star-light.

METEOROLOGICAL JOURNAL KEPT AT BOSTON,
LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1816.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Aug. 15	22	64·5	29·57	Fair—showers in the evening
16	23	62·	29·70	Heavy rain
17	24	58·	29·85	Showery
18	25	58·	30·12	Fair—showers in the evening
19	26	59·	30·30	Cloudy
20	27	63·	30·26	Fine
21	28	61·	30·30	Ditto—rained at 6 P. M.
22	29	61·	30·20	Cloudy
23	new	62·	30·25	Ditto
24	1	62·	30·25	Showery
25	2			Cloudy
26	3	65·	30·33	Ditto } No rain
27	4	60·	30·32	Ditto }
28	5	65·5	30·30	Very fine
29	6	60·	30·23	Cloudy
30	7	59·	30·	Ditto
31	8	57·	29·37	Rain—violent storm at night from the S. E.
Sept. 1	9	54·	29·55	Showery—gale from the N. W.
2	10	53·	29·80	Cloudy but fair
3	11	53·5	29·85	Very fine
4	12	53·	29·50	Rain accompanied by a storm of hail
5	13	53.	29·75	Fine
6	full	54·	30·50	Cloudy—rain in the evening
7	15	60·	29·95	Very fine
8	16	58·	29·72	Cloudy
9	17	57·	29·68	Rain
10	18	60·	29·70	Fair—blows hard from W.S.W.
11	19	63·	29·94	Very fine—showery in the evening
12	20	57·	30·12	Showery
13	21	57·5	30·26	Cloudy—showery in the evening
14	22	68·5	30·16	Very fine

There has been much less rain this month than the last. The harvest is
now begun generally in this neighbourhood.

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand

For September 1816.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.			
Aug. 27	52	63	54	30·17	50	Fair
28	54	63	50	·19	42	Fair
29	48	60	55	·10	37	Fair
30	52	58	50	29·90	49	Fair
31	50	55	49	·10	26	Stormy
Sept. 1	49	57	47	·39	0	Rain
2	46	55	46	·68	31	Fair
3	42	55	50	·73	33	Showery
4	46	58	51	·40	29	Hail storms
5	46	59	49	·72	36	Fair
6	47	60	56	·96	32	Cloudy
7	48	64	56	·93	47	Fair
8	57	66	57	·89	54	Fair
9	57	60	57	·60	0	Rain
10	58	64	56	·82	36	Fair
11	55	64	52	·90	40	Fair
12	50	65	52	30·05	57	Fair
13	53	66	60	·17	54	Fair
14	60	69	60	·10	57	Fair
15	61	70	57	·07	58	Fair
16	56	75	58	·09	56	Fair
17	50	68	56	·01	45	Fair
18	56	60	55	·02	33	Cloudy
19	55	60	50	·05	52	Fair
20	50	60	54	·06	46	Fair
21	54	59	53	29·69	37	Cloudy
22	55	67	54	·70	48	Fair
23	50	63	55	·72	55	Fair
24	55	62	55	·72	27	Showery
25	55	64	54	30·00	32	Fair
26	50	64	55	·12	35	Fair

N.B. The Barometer's height is taken at one o'clock.

L. *New Outlines of Chemical Philosophy. By* Ez. Walker, *Esq. of Lynn, Norfolk.*

M. De Luc has favoured the philosophical world with a new instrument, which he calls The Electric Column. The effects of this instrument are very wonderful; for, if we look upon it merely as a philosophical toy, it excites our astonishment nearly as much as either magnetism, electricity, or galvanism did, when they were first discovered: but when the electric column is used as a philosophical instrument, it may tend to enlarge the bounds of chemical philosophy; for it has happened, and not unfrequently, that improvements in science were at a stand, until some new improvement was made in the arts.

When we investigate the properties of physical elements, which produce such an infinite variety of physical effects, we should be very circumspect in our operations, and not content ourselves with a single experiment, which may seem to support a favourite hypothesis, but vary our experiments; and if a great number of them, made with appropriate instruments, prove the truth of the same hypothesis, that hypothesis may be deemed a theory, and used as such in all our future investigations.

The only means we have of investigating physical causes, is by experiment and observation; for physical certainty does not admit of mathematical demonstration; it is entirely obtained by our senses. We see the sun, and feel his effects,—we view the moon and all her various changes; but the existence of those objects cannot be mathematically demonstrated: all the knowledge we have of their existence is wholly derived from our senses. Hence all our experimental knowledge comes under the denomination of *physical certainty*.

" In ascending from effects to causes we must ever arrive, upon whatever hypothesis we proceed, at some first cause, which does not admit an explanation from mechanical principles."[*]

Now the first causes or elements, by which the various phenomena in nature are produced, are three; that is to say, gravity, magnetism, and electricity:—gravity is a simple element, but the other two are compounds. Each pole of a magnetic bar contains a different element. In two bars homogeneous poles repel, and heterogeneous poles attract each other. And the same law obtains in electricity; for, what have been called positive and negative electricity are two distinct elements;—electrical

elements of the same kind repel, and contrary elements attract each other.

In a former paper I described some improvements that I had made in electrometers, and in the modes of insulation. As these improved instruments are highly necessary in conducting the following experiments, I shall give an account of them here, to save the reader the trouble of turning to my former paper.

The electrometer which I make use of consists of an eight-ounce phial with a stopple of sealing-wax fitted into its mouth; through this stopple a piece of thin music wire passes, and extends down the axis of the glass about 2¼ or 3 inches; the top of the wire standing 3 or 4 inches above the top of the phial. The lower end of the wire is turned into a small ring, from which two slips of Dutch metal are suspended; these are about 3-4ths of an inch in length and 1-10th in breadth. The wire is fixed into the stopple; but the stopple itself is not made fast into the mouth of the phial, but left so as to take out occasionally.

My insulating stands are made thus:—A piece of thermometer tube five inches long is fixed to the bottom of a wine-glass (the top being broken off) with black sealing-wax, and a piece of a stick of the same kind of wax, about an inch and an half in length, is fixed upon the top of the tube. The top of the wax being made soft, is formed into a proper curve for glass tubes to rest upon. This form is very convenient for making experiments with electric columns; but a circular piece of glass, having its upper surface gilt with gold-leaf, fixed upon the top of the wax, is more convenient for many other experiments*.

Description of a Silver-leaf Electrometer.

The object of this instrument is to investigate some properties of M. De Luc's electric column. The electric machine collects two elements by friction; the Galvanic apparatus produces the same two elements by a chemical process; but the electric column, by some unknown process, produces these elements without either friction or chemical action.

In fig. 1. *ab* represents part of a thermometer tube fixed into the base of the instrument at *a*. *bc* a cylinder of black sealing-wax, rather thicker than the tube; and *cd* a strip of thin crown glass, about an inch and a quarter in length and a quarter of an inch in breadth. To the lower end of this glass, a slip of silver-leaf† *dp* is fixed with gum-water; *rq* is another slip of silver-

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* See Phil. Mag. vol. xlvi. p. 210, for a further account of these insulating stands.

† There is an article manufactured in imitation of silver-leaf, which answers much better for this purpose than that which is genuine.

leaf

leaf of dimensions equal to the former, suspended from the lower end of the wire *w r*. *x y z* is a piece of thick music wire, one end of which is fixed into a peg of hard wood inserted into the base of the instrument at *x*; but the rest of the wire is detached, and vibrates freely with the least force impressed upon it. The glass cylinder, standing in a groove turned out of the foot of the instrument, protects the pendulums *d p* and *r q* from being disturbed by currents of air.

The thermometer tube *a b* and one end of the wire *x y* are fixed into the base on the outside of the glass cylinder: the other end of the wire *y z* and pendulums are suspended within it.

To the lower ends of the pendulums *p* and *q* two pieces of thin wire are attached, of the forms · represented in fig. 2.

Fig 2

Fig 1
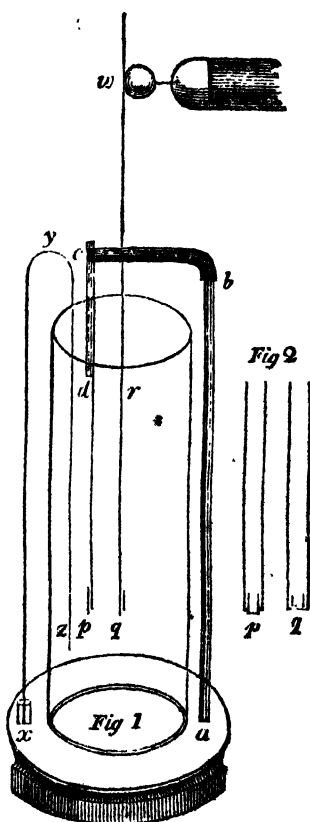
Exp. 1. An electric column being laid upon two insulating stands in an horizontal position, an electrometer placed in contact with the zinc end of it exhibited only a few degrees of electricity, nor was any greater effect produced on the electrometer when it was applied to the copper end. Hence it appears that the column contains very little electricity in an insulated state.

Exp. 2. The electrometer remaining in contact with the copper end of the column, as soon as a communication was made between the zinc end and the earth by means of a piece of wire, one end of which stood upon the table, with its other end in contact with the end of the column, the leaves of the electrometer diverged to an angle of thirty degrees. This electrometer being set aside, and another equally sensible placed in con-

tact

tact with the zinc end of the column, it diverged to the same angle, the wire of communication having been removed to the other end. Hence it appears that the elements of electricity came either from the earth or the table, through the wire which formed a communication between the table and the column, and that the two elements are of equal mechanical forces, for the two electrometers were found to be in contrary electrical states. That element which was received from the copper end of the column was thermogen (positive electricity), the other electrometer received the contrary element from the zinc end *.

Exp. 3. Having placed a silver-leaf electrometer in contact with the copper end of an insulated electric column, and a communication being made between the table and the zinc end, the pendulums vibrated 140 times in a minute. These vibrations were performed thus: As soon as the pendulum *rq* became charged by the column, through the wire *w* acted and charged the pendulum *dp*. The two pend ing now charged with the same element repelled each other; *rq* diverged to the right, *dp* to the left, and discharged its contents into the wire at *z*, which conveyed it to the earth. The pendulum *dp* being now reduced to its natural state, was again attracted, charged, and repelled by the pendulum *rq*: thus the vibrations of the two pendulums were continued. But as soon as the wire of communication was removed from the zinc end of the column, the pendulum ceased to vibrate: hence we may conclude that the element which put the pendulums in motion ascended up the wire at the zinc end, and passed through the column to the copper end.

The electrometer being placed at the zinc end of the column, and the wire of communication against the copper end, the pendulums performed the same number of vibrations in a given time as before, but they ceased to vibrate as soon as the communication was cut off between the copper end and the table. The pendulums were put in motion at the copper end by thermogen, the element of heat; but at the zinc end, photogen, the element of light, produced an equal effect.

Exp. 4. Two silver-leaf electrometers, whose pendulums were of equal lengths, being placed in contact with an insulated electric column, one at each end, began to vibrate at the same time, and the pendulums at the zinc end performed the same number of vibrations in a given time, as those at the copper end.

* Mr. Singer observes, in his Elements of Electricity, that " the electrometer connected with the zinc extremity of the column will be *positive*, that connected with the silver extremity will be negative."

Mr. Singer was probably led into this error by using Mr. Bennet's electrometer, which is a very imperfect instrument.

Now

Now as these electrometers were put in motion by two different elements, which afterwards descended to the earth at each end of the column, and as I have four pendulums which have thus been kept in motion for more than a month; the question which remains to be solved is, Does the column generate these two elements as fast as they issue from its ends and descend to the earth, through the electrometers? or, are they derived from some other source?

From the third experiment it appears that the pendulums of an electrometer, placed in contact with one end of a column, do not vibrate until a communication be made between the other end and the earth; and hence we may infer, that the element which keeps one of the electrometers in motion, ascends through the other at the other end of the column, and thus a double current of the elements is passing through each of the electrometers.

This is not, however, a mere conjecture; for when the pendulum z is placed without the sphere of attraction of the pendulum $r q$, let the end p of the pendulum $d p$ be moved by means of a glass rod, till $r q$ attracts, charges, and repels it. When p strikes the wire at z, it discharges the element which it received from q, and receives the contrary element from the wire. The pendulums being now in a contrary state, attract each other, and the vibrations are continued; but let the pendulum $d z$ be touched with a piece of wire, to deprive it of the element which it received from the wire at z, and the pendulums will instantly cease to vibrate.

Exp. 5. An electrometer, containing two pendulums which are each about three inches in length, made as represented in fig. 2, was placed in contact with the zinc (negative) extremity of a series of columns; and as soon as another electrometer with pendulums, each about one inch in length, was placed at the other extremity, the four pendulums began to vibrate. The long pendulums vibrated 140 times in a minute, and struck the wire at z with so much force as to produce a fine musical tone, which was loud enough to be heard at the distance of 26 feet. And these pendulums vibrated an equal number of times in a minute, and produced the same strength of tone, when they were removed to the copper extremity of the series: the short pendulums being removed to the other extremity vibrated as before, about 300 times in a minute.

Lynn, Sept. 13, 1816.　　　　　　ÉZEKIEL WALKER.

[To be continued.]

Ll. *Letter to the Right Honourable the Countess of Gosford, on the Similitude, and Difference, in the original Formation of the Island of St. Helena, and the Basaltic Districts in the County of Antrim; with the Similitudes and Differences of the posterior Operations of Nature performed upon each. By* W. RICHARDSON, D.D.*

Wᴴᴇɴ your ladyship persuaded me again to take up my geological pen ; and, as an inducement, permitted me to address my speculations to yourself, I intended to have limited them to the facts exhibited in our own country, part of which I had the honour of showing to your ladyship.

I then intended to proceed to the application of these facts, and others within the reach of your own observation, to the theories maintained by the Neptunian philosophers, to which your ladyship had recalled my attention, through some recent publication of some celebrity.

I find, however, that I cannot stop, but must avail myself of your permission to carry your ladyship to more distant regions : —drawing general conclusions both from the magnificent scenes I led you through, and from the corresponding features of a remote island, exhibited on a still grander scale.

The similarities of these countries so distant from each other, and the facts establishing them, shall form the subject of the present letter: while the greater part of the next shall be limited to the conclusions fairly drawn from them. But when rigid demonstration can no longer be obtained, I hope your ladyship will excuse me for indulging my imagination, and wandering into the regions of probability, and even of conjecture.

I am, with much respect,

Your ladyship's most obedient humble servant,

Clonfecle, Moy, May 31, 1816.　　　W. RICHARDSON, D.D.

St. Helena.

It is now some years since my ingenious and philosophic friend Dr. Macdonnel sent me a small volume he had lately received from London, which bore strongly on a subject that had been the topic of frequent discussions between us, and had given occasion to some pleasant excursions which we made together, with a view to examine with the greatest care and accuracy the scenes where the objects connected with it were displayed in the greatest abundance and to the best advantage.

The little book was entitled "A Description of the Island of St. Helena." It was written by a gentleman who spent some weeks on that curious little spot on his return from India ; and

had

had examined the materials of which the island was formed, and the manner in which they were arranged, with much accuracy.

My author does not claim to be a scientific naturalist, but he is obviously a very acute observer; and though he seems to have adopted, without examination, the volcanic theory of the origin of basaltes, which was in fashion when he left Europe, he possesses a degree of fairness and impartiality which I have seldom met with in naturalists attached to a particular opinion or theory. My friend was induced to send me this pleasant little volume, because the picture it gave of St. Helena bore so striking a resemblance to the features of the basaltic area in the north of Ireland, that had so much occupied the attention of each of us, and to which my own observations on the natural history of my country had been nearly limited.

It was not only the similitudes in the original arrangement of each of the districts, which were numerous and obvious, but also the ——s, that strongly arrested my attention; the former affording demonstration of the exact similarity of original formation, while the latter proved irresistibly that the posterior operations of nature, which have taken place in St. Helena and in our part of Ireland, though in some instances the same, are very different in others.

Hence a new field is opened, and new materials afforded for geological discussion, particularly interesting to me, as the facts which I observed in my own country, that led me to sustain positions sometimes deemed paradoxical, are not only exhibited in equal abundance in St. Helena, but also so diversified as to afford new arguments and further demonstration of the truth of those positions.

That island has now acquired a new interest—the eyes of the world are directed towards it and its new inhabitant. The Latin poet seems to allude to this sequestered spot, as now colonized, which he describes,

> " Ut maris Ægæi rupem, scopulosque frequentes
> Exulibus magnis——"

While the English nation is making such exertions to secure the comfort of this great exile—let us find amusing employment for him; let us direct his attention to the natural history of his new country, and we shall probably protect him from the misery into which the exiled Ovid fell; and from disgracing himself by unmanly complaints, as that poet did, on the harshness of a climate nearly as mild as that of St. Helena itself.

I too have prepared a source of entertainment for him, as my pupil Colonel Wilkes, the late governor, after showing the great success with which he raised Fiorin grass at Madras, since his

Q 4   appointment

appointment to the government of this island, not fruitful in provisions either for man or his domestic animals, has taught its inhabitants how to supply the wants of the latter most abundantly, by the introduction of this valuable grass, of which he has lately sent home, to be transmitted to me, some magnificent specimens raised by himself in this new field.

The emperor Diocletian, a mighty conqueror also, after wielding his sceptre a long time over nearly the same territory with the emperor Bonaparte, found content and amusement in the culture of his cabbages.

Let then his successor take cordially to the culture of the vegetable he finds just established in St. Helena, and he will pass the remainder of his time smoothly, showing the world, quite contrary to their expectations, that he will end his days in peaceful amusement, and that

> " Fnem anima quæ res humanas miscuit olim
> Non gladii non saxa dabunt non tela."

I shall now return to the natural history of this curious little island, and shall endeavour to trace the exact similitude in original construction between our own basaltic district and this remote spot.

The first and most striking point of resemblance seems to be in the accurate stratification of both countries, and the sameness of the arrangement of the same materials in each.

Our author, in his preface, tells us of the " horizontal beds of basaltes;" and in his 52d page, " stratified appearances of the declivities of the hills, consisting of layers which rise one above another."

" All the matters of which the island is composed are placed in beds, various in their depth, colour, and texture."

" On the steeper declivities, the projecting ends of the strata resemble flights of steps rising above one another."

Could I have given a more accurate description of the arrangement of the strata, so beautifully displayed, in both the perpendicular and steep precipices lining so much of our northern coast?

Our author talks of the terraced form of St. Helena.—Terraces are common with us.—The island of Rathlin is a mass of terraces; this form arises from original stratified construction, and posterior removals.

The leading, and I believe the sole material in each, is basaltes; a fossil upon which Nature seems to have impressed a peculiar character, wherever she has been pleased (as often) to form a distinct area of this curious stone.

Our author tells us: " the rock which forms the principal
strata

strata of the island appears evidently to be basaltes; it is always regularly fissured, and running in distinct layers; these layers have always somewhat of a columnar appearance."

" The front of the columns is sometimes flat, but more generally prominent and angular."

Again, "We sometimes find series of columns of equal height, resembling a piece of artificial work."

Are not these series the beautiful groups so abundant with us, to which the country-people give the name of organs?

There is not any circumstance in our Antrim façades which has struck me more than the general tendency of the basaltes to assume a columnar form, though often very imperfect; and this not from decay, but from failure in the original effort to obtain greater regularity.

The same effort of Nature is observable at St. Helena:—" In the most irregular masses we can always observe a tendency towards this columnar form."

The next material I shall mention common to St. Helena and Antrim, and disposed exactly in the same manner in each, is OCHRE; this bright red substance Mr. St. Fond sustains to be basalt which has undergone some chemical process of Nature with which we are not acquainted. In this opinion I have acquiesced: but my author calls it clay, and it seems equally abundant and similarly arranged in both countries.

" Numerous layers of clay; that of a bright red is the most common, often seen in layers of only a few inches thick; these red veins traverse the whole island."

This red matter, with my author *clay*, with me *rock*, is disposed everywhere as with us: " In the heart of the rock we find nodules of clay, and among the clay nodules of rock."

There is not any circumstance on our whole coast, that always struck me more forcibly, than the transitions of our strata into each other in a vertical direction; for though there be a great difference between the component rocks, both in material and in the principle of internal construction, yet they invariably pass into each other nearly *per saltum*, and we never find the solidity or continuity of the so different materials interrupted. Such, too, seems to be the style of the junctions of the strata at St. Helena—" The rock in some places terminates, above and below, in indurated blue or black clay, continuous with it; but passing so insensibly into it, that we cannot discern at what point the stone ends or the clay begins."

I discovered a very curious fact, sometimes, though rarely occurring with us, in a few distinct strata; that is, cavities; some *now*, the rest probably *once*, filled with pure fresh water, as in a quarry or stratum, open at Ballylagan, the stratum at Islamore, and

and the 11th stratum counting from the water at Bengore pro-
montory:—in the debris from this stratum, both at Portmoon,
and near the causeway, on its west side, I find such cavities; but
at the causeway itself, or its more contiguous strata, I never found
either water or internal cavity:—at St. Helena this singular fact
also occurs, but seems still more rare—" In a quarry, the stone
when broken is found to have many large internal cavities,
which contain a pure and wholesome water, shut up in the body
of the rock."

The most striking feature of resemblance between the basaltic
districts of St. Helena and Antrim, is to be found in the whyn
dykes, so common to each;—these mighty walls, which seem
peculiar to basaltic countries, though they are often found to
extend and diverge into districts formed of different materials,
seem to have excited little notice until very modern times: even
Dr. Hamilton, in his celebrated Letters, which gave the first philo-
sophical account of our curious coast, very slightly notices those
contiguous to Ballycastle; letting all the others issuing from the
precipices on the east and west of the Giant's Causeway, and
burying themselves in the sea, entirely escape him, though more
magnificent and more decidedly marked than those he men-
tions; nor did he examine in those he notices their singular in-
ternal construction, the consummate regularity of their masonry,
not less wonderful than their external wall-like forms.

I have always considered our whyn dykes as more curious
than our prismatic and columnar groups, which seem hitherto
to have absorbed most of the attention given to our wonderful
coast;—regular internal arrangement is not peculiar to basaltes,
nor are the vertical prisms and pillars forming our magnificent
colonnades more wonderful than the equally regular horizontal
prisms of which our whyn dykes are constructed.

For an account of the whyn dykes on our Antrim coast, I
must refer to the Transactions of the Royal Irish Academy for
the year 1802. These dykes, when I gave in my Memoir on
that subject, I thought sufficiently grand; but how insignificant
do they now appear, when compared with the St. Helena dykes,
or with those more recently discovered on the rocky mountains,
the range that divides the vast American continent, the *divortia
aquarum* whence the waters are poured from their elevated
sources in opposite directions to the Atlantic and Pacific oceans,
at a distance of some thousand miles !

Of the grandeur of the latter dykes we have sufficient evidence,
but want particulars; the St. Helena dykes are well described
by our author; he calls them " huge vertical strata of broken
and fissured rock, which traverse the whole, from the base to the
summit."

I must

I must here observe, that wherever our author uses the word *fissured*, he means the divisions into pillars or prisms; that is, the basaltic arrangement, common both to dykes and façades.—He proceeds: " Resting on the summit of these hills, we see huge detached masses of rock, which rise several hundred feet above them:" again, "One observes here, besides the horizontal and parallel strata, that they are all penetrated by huge perpendicular strata of loose and fissured rock."

" With respect to the perpendicular strata—they are often of great breadth, and all regularly fissured ; the fragments quite separate and distinct; but as uniformly fashioned, and evenly placed, as the stones of a building."

" Several of these vertical strata rose considerably above the plane of the hills which they penetrated, and presented the appearance of huge walls of stone, surmounting their summits, and descending along their declivities to the base."

There appears to be an exact similarity between the prismatic stones of which the St. Helena and Antrim dykes are formed.

" The fragments which compose them are of all sizes, some of them being six or eight feet long, others only a few inches, but so regular and smooth, that they seem well adapted to the purposes of masonry, without the aid of the chisel or hammer."

I have stated the pillars of our Antrim colonnades to be formed by the accumulation (in a vertical direction) of prisms, exactly similar ; but that these have no internal principle of construction, the great joint breaking irregularly, and with a conchoidal fracture—while the great prisms of which our dykes are formed, and laid as it were by a mason, in a horizontal position, have a subordinate principle of construction, breaking, not like the others, with a conchoidal fracture, but into smaller prisms, already formed, with their sides brown and polished; and I call, for distinction, these two descriptions, component and constituent prisms, a style of construction peculiar to whyn dykes, and which extends also to those in St. Helena, as appears clearly from the foregoing passage.

Our author tells us ",of masses of irregular rock cemented together with a ponderous lava." Nothing commoner with us than masses of sound basaltes, cemented together by a sort of solid basaltic mortar, the fracture of the former, blue ; of the latter gray, and granular ; such is the mass of rock upon which Dunluce castle stands.

It cannot be deemed extraordinary that districts formed by Nature of the same materials, should have these materials similarly arranged in each ; but where we find differences, we are led to inquire whether they arise from a diversity in the original formation, or are the result of posterior operations ; and if we

we suspect the latter, by diligent attention we may perhaps be able to trace the effect to the cause, and so be able to establish the existence of these posterior operations, and their manner of acting.

The striking difference between the present appearance of St. Helena and the basaltic of Antrim, seems to consist in the superior magnificence of the dykes of the former, and their exhibition of their real forms, by their immense elevation above the surface in their proper shape—while our dykes, as accurate walls as the other, and also of prodigious height, rarely emerge from the stratified materials they cut through, so as to show their real form of wall to a careless observer; yet in some few instances they do exhibit themselves as actual walls, rising above the surface, and pointing in the direction of the greater remains of the wall, unequivocally displayed in the contiguous façade, as at Port Cooan and Portnabaw.

Another material difference occurs between St. Helena and Antrim.—I have sustained that the latter has never been the seat of volcanic fires, nor exhibits any marks of having been acted upon by that powerful element;—while the marks of dreadful combustion are unequivocal over the whole island of St. Helena. Our author tells us:

"The structure of St. Helena seems to demonstrate that it is the work of subterraneous fire."

"The ancient seat of volcanic fires, and subterraneous explosion."

How have these fires operated? Not by that instrument we call a volcano, whose mode of acting is quite familiar to us, ever since Sir W. Hamilton has been so particular in his account of our two great European volcanoes:—the combustion of St. Helena has been general; fire has acted violently upon its whole surface, but its intensity seems gradually to abate as we ascend from the water edge to the highest point of the island; at a low level the matters are so scorched and scorified, as not to admit degradation and decomposition into vegetable mould; hence the lower parts of the island are black and torrified, naked and barren, incapable of sustaining plants; but as we ascend, vegetables begin to appear; and where the elevation is great, the verdure becomes splendid.

Then we are told—"the exterior parts of the island, all round where they border on the sea, present the appearance of a burnt and scorified shell, black, rugged, and mouldering, without the least trace of vegetation."

"Some of the interior ridges of hills, which are much higher, are covered with verdure."

"The central ridge is covered to the summit with the most
luxuriant

luxuriant herbage, and with groves of indigenous shrubs and trees—lower down we observe numerous groups of argillaceous hills, all perfectly naked; it is indeed near the water's edge, and under its surface, that we find the largest masses of lava, and of volcanic cinders and so."

We have abundant proof   wn country, that decomposed basalt produces a beautiful   at the greatest elevations;— the little valleys lining the sides of our most elevated basaltic façades, at Magilligan, Cave Hill, and Monyneeny, are of a brilliant green; and the high verdure of the steep precipices lining Bengore promontory, strikes every one; yet these green declivities are rarely cheered by the rays of the sun, from their northern exposure, and approach to perpendicularity.

In St. Helena, the basaltes and burnt matters are mixed in a manner that must be very embarrassing to those who undertake to account for the formation of this singular island; and few naturalists can refrain from indulging their wise conjectures.

Our author says, "All these layers consist of rock, placed alternately with deep beds of volcanic matters; this rock is evidently basaltes."

"The parallel and horizontal strata leave a wide intermediate space, which is occupied by an irregular mass of agglutinated volcanic matter."

"Frequently eight or ten ascents of rock are separated by these volcanic masses."

This steady alternation of basaltic strata with scorified matters, evidently burnt, bears no resemblance to any thing yet observed at any of our known volcanoes. Our author, though he endeavours to account for this arrangement by successive eruptions, is startled when he can discover no remaining craters. Mr. Demarets too, when he attempted to account for the basaltic colonnades in Auvergne, as produced by volcanic eruptions, admits that in many cases the craters had entirely disappeared, and in others that the currents of lava had vanished. And our author tells us expressly, "that in the island itself there are no sulphureous, bituminous, or inflammable matters."

In my different memoirs on the subject of our basaltic country, I had repeatedly asserted that it did not afford a particle of burnt matter, scoria, or cinder; and that our solid basaltic rocks did not exhibit the slightest trace of having ever sustained the action of fire;—of late, however, a discovery has been made of some quantity of cinders, and scorified matter; small in   but the fact becomes important, when we find that these matters are disposed in the very same manner in which our author found similar matters, to an immense amount, at St. Helena; that

that is, between the strata of columnar basalt, alternating with them in every variety of thickness.

I have in the Transactions of the Royal Society given a minute list of the strata composing the promontory of Bengore. The magnificent stratum forming the upper range of pillars at Pleaskin, is the tenth, counting from the sea—this is surmounted by another stratum of massive pillars (the eleventh,) of greater diameter, coarser material, more imperfect workmanship, and much shorter.

We have different views of portions of these strata, from Dunseveric to the cascade at Portmoon, where the upper one is taken away for a mile; it appears again at the depression west of Pleaskin, and is often seen for another mile, always resting on the tenth; and wherever the junction is exposed, these strata pass into each other *per saltum*, without interrupting the solidity or continuity of the material: so that I have no doubt, could we quarry (as we often do at the junction of other strata), the stones would not break at that junction, but we would find masses containing the junction, and portions of each stratum adhering solidly.

After being thus connected for nearly three miles, without any interruption of their continuity, as they arrive at the western point of Port Knoffer, almost immediately above the Giant's Causeway, they are separated for a short space by a layer of scoria and cinders placed between them, as happens so often at St. Helena.

There is another remarkable feature in which St. Helena exhibits strong marks of having been acted on by intense fire, to which I find nothing similar in our basaltic country;—the heat has been so violent between the basaltic strata, as to act on the ends of pillars terminating them, without affecting the middle of the strata.—Hear what our author says:

" The middle of the rock, where it has not been injured by time, or the effects of fires."

" We can generally trace somewhat of the columnar appearance yet, from the scorification of their bases and summits."

" The summits and bases of the basaltic rock are always more or less scorified, cellular, and honeycombed."

" The bases and summits of the columns are so black and scorified, that they look like trunks of trees burnt to charcoal at each end."

These facts are of extreme importance, when we direct our speculations to original formation and arrangement; for, first—in respect to the long received opinion, that basaltic pillars are of igneous formation: it now appears, that in St. Helena, the action of intense heat, *in situ*, has tended, so far as it could
reach,

reach, to injure and deface them; and as to the violent vol-
canic explosion, by which our author supposes this island may
have been raised at once from the bottom of the sea; the facts
I have just quoted are utterly irreconcilable to such supposition,
and would rather lead us to a long train of alternate operations;
violent ignition, regularly followed in succession by gigantic ba-
saltic formation, and the result of mighty accumulation of steady,
parallel strata, at length laid bare by posterior operations, re-
moving much, but disturbing nothing, and disclosing in the vast
precipitous façades of the island, the early arrangements of Na-
ture, with the materials still retaining the positions in which
they were originally placed.

I shall now proceed to try if the new matter furnished by this
singular island affords any additional support to the wild posi-
tions I have already sustained, to wit—that our *present*, is not
the *original* surface of our globe, but much less elevated, and
greatly diversified by the action of powerful agents, with which
we are not acquainted.—That these agents have carried off im-
mense masses of our original materials without disturbing what
they left behind—and that the accumulations of our strata once
reached higher than the summits of the loftiest mountains I have
had it in my power to examine.

The facts from which these positions follow as conclusions,
must be reserved for another letter, in which I mean to gene-
ralize; and after proving that these positions receive the fullest
support from the facts found in St. Helena, I shall probably
extend my views, and show that similar operations have been
performed on other parts of the world, which I have not ex-
amined.

And what may appear yet more wild—that there is a portion
of the world still reserved for similar operations.

<div align="right">W. RICHARDSON, D.D.</div>

LII. *On the Excitement of Voltaic Plates; in Reply to Mr. De
Luc's Objections to the Doctrines maintained by the Author.
By* J. D. MAYCOCK, *M.D.*

[Concluded from p. 172.]

IT has never been my intention to propose a new hypothesis for
explaining the excitement of the Galvanic pile: but as Mr. Volta's
(differently modified) appeared to be very generally and impli-
citly received, I noticed in my Essay some objections to which
I considered it liable; and as it will not take up much of the
time of your readers, it may not be amiss to touch on that sub-
ject again.

<div align="right">The</div>

The fundamental principle of M. Volta's hypothesis is, that from the contact of two metals the equilibrium of electricity is destroyed, and the metals while in contact possess different states of electricity. This principle was supposed to be fairly deducible from the phænomena of the Voltaic *plates.*

In the first place my experiments demonstrate that two plates of dissimilar metals, not previously electrified, being brought into contact, indicate no electricity whatever; but on being separated a change in their electrical state is immediately perceptible, the one being *positive* and the other *negative.* A supposition that two bodies are in different states of electricity while in contact, because they are so after separation, would certainly be gratuitous; but to affirm that two bodies, which indicate no electricity whatever, are in different electrical states, would be nothing less than to contend for an absurdity. In the combination for a Galvanic pile the metals are brought together, they remain in contact a considerable time, and during that time the excitement of the pile commences and continues. How can the excitement which appears in the *pile* while the metals are in contact, be referred to the same principle that occasions the excitement of the *plates*, which never indicate excitement while in *contact*—never until they are *separated?* In the second place, Mr. De Luc has clearly demonstrated that the efficient group consists of two metals, not in *contact*, but with a moist substance *interposed.* How then can the excitement be attributed to an electro-motive property of metals, exerted while they are in *contact*, when contact does not appear to be an essential to excitement;—nay, when excitement is not evinced in the case where contact subsists between the metals, as in dissection the third; but is fully evident in that, in which contact does not subsist, as in dissection the first? In the third place the Voltaic *plates* do not act unless they are applied to each other at so many points as to manifest a sensible cohesion; but in the *couronne de tasses*, the porcelain troughs, and Mr. De Luc's *first dissection* of the pile, excitement is produced, although the different metals are connected by very little more than a physical point. Again; the Voltaic *plates* do not act unless their surfaces be perfectly bright and dry; in the *pile* the metals, even when new, are never so clean as the Voltaic plates; in general they are very much tarnished, and during their action are never dry. Is there then any analogy between the circumstances necessary for the excitement of the *pile* and those necessary for the excitement of the Voltaic *plates?* and how can the phænomena of the Voltaic *plates* afford a *first principle* for explaining the excitement of the Galvanic *pile?*

In thus arguing against the hypothesis of M. Volta, I by no
means

means wish to be understood as supporting the chemical expla-
nation which has been opposed to it. I am aware that it is,
in its turn, liable to some objections. Nevertheless it appears
to me that a decomposable fluid (moisture at least) is always
present during excitement of the pile, and that chemical ac-
tion is in some way essential to it. In what manner the de-
composable fluid acts in producing excitement, I think still a
question; and on this account I have observed that all the opi-
nions which have been proposed to account for the excitement
of the Galvanic pile, the Galvanic trough, or the electric column,
are extremely unsatisfactory.

Having laid particular stress on the fact that the contact and
separation of dissimilar metals occasion them to assume different
electrical states, that the difference is not observable while the
bodies are in contact, but becomes evident on their separation;
and having also stated that excitement is not produced by the
contact and separation of plates of similar metal; and further,
that the law is, as far as experiment has gone, general, and not
confined to metallic bodies[*],—I ventured to remark in a second
paper[†] on the subject of electricity, that the contact and sepa-
ration of successive particles must take place during friction; and
therefore, that if the phænomena of electricity, as produced by
friction, could be consistently explained on the principles which
apply to the excitement of bodies by contact and separation,
then there would be a fair ground for generalization. By at-
tempting such an explanation I endeavoured to establish, as a
position, that the contact and separation of dissimilar bodies is a
cause of electrical excitement, and that the excitement by fric-
tion is referable to this property. I did not, however, attempt
to carry the generalization further: the cause of excitement of
the Galvanic pile I do not consider to be yet known, wherefore
it must be impossible to ascertain what relation it bears to the
cause of excitement by contact and separation. In the rapid
progress of investigation it will no doubt be discovered that the
two causes of excitement, contact and separation, and combina-
tion (if I may so express the means of exciting the pile), are both
referable to one cause, and then the generalization will be per-
fect, as far as relates to the facts which at present constitute
electrical science. Thus, as knowledge accumulates and reason
is successfully exerted, the principles of science become less nu-
merous and more general.

Mr. De Luc, however, objects to the generalization I have at-
tempted on two grounds:—the first, the supposed inaccuracy of
my experiments, concerning which I shall add nothing to what

[*] Phil. Journal, vol. xxix. See Experiments.   [†] Ibid. vol. xxxi.

I have already said ;—the second, the result of his own experiments, which I shall presently consider.

Before I proceed, let me observe, that although I certainly have used the word *electric* in opposition to *conductor*, it was by no means my intention to imply that "the former only had the faculty to be excited by friction;" and lest my mode of expression should appear to others as well as to Mr. De Luc, to involve such an hypothesis, I would wish substituted the word *non-conductor* for *electric* wherever the latter occurs in either of my papers.

The experiments, from which is formed Mr. De Luc's second ground of objection, are contained in a paper on the electric effects of friction*, and tend to prove that friction between similar bodies will produce excitement. Certainly as excitement takes place from the contact and separation of dissimilar bodies, and does not from the contact and separation of similar bodies†, the analogy I have endeavoured to establish will not hold, except it appears that electricity is excited by the friction of dissimilar bodies, and is not by the friction of similar bodies. That no considerable degree of excitement can be obtained by the friction of similar bodies, will I suppose be readily admitted; and that the fact is generally true, that it is the friction between dissimilar bodies that produces excitement, the few instances to the contrary being only exceptions, will also be granted; and I might therefore urge, that it is not fair to bring a few anomalous exceptions to a general rule, in opposition to strong analogies. Let me however observe, that if two bodies be precisely similar in relation to an experiment, and if they be made to act on each other, their action must be reciprocal,—in that action the acting surfaces must both be submitted to a like operation, and it is altogether impossible to conceive how the effect on one can differ from the effect on the other. Moreover, electrical excitement is, as far as we know, confined to the surface of bodies. The influence of dissimilarity of surface in reference to electricity is sufficiently known, a difference of colour will occasion a body to assume a positive or a negative charge. If a polished tube of glass be excited by friction with flannel, it will be positively electrified; but if the surface of the same glass tube be ground and submitted to the same operation, it will be negatively electrified. Supposing Mr. De Luc's experiment to have been performed when the apparatus was new, and when the polish on the glass rubber and on the glass cylinder was perfect, a very little difference in the hardness and fineness of the two pieces of glass would occasion the surfaces to be differently affected when he

* Phil. Journal, vol. xxviii.   † See my Experiments. Ibid. vol. xxix.

" turned

" turned rapidly the winch for a little while." I doubt not that a polished glass surface would easily be excited by a ground glass surface : for as the grinding of glass so alters the relation it bears to woollen cloth, that in its polished state it becomes positive, and in its rough state it becomes negative, by friction with it,— so do I suppose that the grinding of one of two pieces of glass will so alter their relations, in point of electricity, to one another, that friction between them may occasion excitement. Again: there is a great variety in glass, arising from the materials of which it is manufactured, and we are not satisfied that the pieces were in the first instance precisely similar. I think also that it might be shown that in the experiment with the ribbons (analogous to those performed a long time since by Mr. Bergman with skains of silk), from the manner they were made to act on each other, there was some difference as to surface. I will not, however, urge this point any further: I would not wish that the analogy I have attempted to draw, should be supported by finely drawn ingenious suppositions; but that, if it be found to hold generally true, it should not be rejected on account of one or two anomalous exceptions:—and if it be ascertained that all considerable degrees of excitement by friction are obtained by the action on each other of dissimilar bodies, it will not be altogether unphilosophical to suspect that some minute causes, not cognisable to our senses, or not easily understood, have operated, and tended to produce the anomaly in those cases, in which very minute degrees of excitement are observable from the action on each other of bodies apparently similar.

In explaining the views I had taken of some points of electrical science, I cautiously avoided using any expressions which would involve an hypothesis ; and when I was constrained from long usage and the want of better terms to do so, I accompanied those expressions with one or two remarks, which I have been rather surprised to find has been the object of censure. Mr. De Luc considers not only that " the existence of an electric fluid has been demonstrated by a long series of experiments," but that it is composed of " many ingredients," that " besides light, fire, and an odorate substance, there are other ingredients in the electric fluid, one of which well determined is a most tenuous fluid, which imparts its strong expansibility to the others, and is the cause of the phænomena called electric influences." This fluid is called "vector, as giving motion to the inexpansive substance which constitutes the density." Furthermore, that it has been " demonstrated that electric motions are produced only by the substance constituting the density, without any participation of the fluid producing the electric influences." I am fully aware, sir, that Mr. De Luc has written all this, and has

R 2 • instituted

instituted many experiments on the subject:—but I am not satisfied that he has demonstrated it. The species of demonstration to which I have been accustomed, has always carried to my mind a degree of conviction far more irresistible than any thing I have ever seen written on the subject of an electric fluid. I cannot help thinking that the points which are here considered as demonstrated, have been only very ingeniously supposed; the suppositions being grounded on their capability (*if true*) of explaining electrical phænomena. I do not mean to argue that they are not true; it might be as difficult to prove their falsity as their truth; we all know on whom rests the *onus probandi.* Admit them, and probably they will explain very agreeably a number of important and interesting phænomena:—Give to Archimedes a point on which to rest the fulcrum of his lever, and he moves the earth.

Mr. De Luc also seems to think that the rejection of the idea of an electric fluid, such as he has analysed and described, involves the whole field of electricity and galvanism in obscurity. Now I cannot possibly conceive how pausing a little before we enter into the mazes of hypothesis can have such an effect; at the same time I am satisfied that by stepping too boldly into the wide field of speculation we endanger the interest and reputation of science. I do not consider the remote causes of phænomena to be the objects of science; but the generalization of the known properties of bodies, and the discovery of those properties by observation and experiment. Thus, I suppose the science of physical astronomy complete, although the remote cause of *gravitation* remain for ever unknown; and the science of pneumatology has been carried to a high degree of perfection by those authors who have made no reference to the essential qualities of *spirit;* and I cannot perceive an absolute impossibility for all phænomena, termed electrical, whether they become evident in our confined laboratories or in the great laboratory of Nature, the universe, being generalized and arranged in the form of a science, independent of any curious inquiries as to the existence and nature of an *electric fluid.* However indispensable, it is certainly not sufficient that the causes to which we refer phænomena be capable of explaining them; they must be known to have an existence, before they can be supposed to operate.

" It is a dictate of common sense," says the excellent Reid, " that the causes we assign of appearances ought to be real, and not fictions of human imagination; and it is likewise self-evident, that such causes ought to be adequate to the effects which are conceived to be produced by them." Impressed with this opinion, I do not think it advisable to interweave with experimental facts and legitimate conclusions, hypothetical assumptions,

sumptions, however ingeniously they may have been conceived, or however comprehensive may be their grasp.

Sir Isaac Newton published his *demonstrations* of the universal influence of the law of gravitation, in his admirable work *De Principiis*; his *speculations* concerning an ætherial fluid are thrown together in the form of query at the end of his Optics: but the valuable experiments and discoveries of some philosophers are so blended with hypothetical reasoning, that it is difficult to separate the one from the other; and this may in some way account for their having remained without that degree of attention to which their intrinsic worth entitles them, "*meritis non respondere favorem.*"

From the preceding remarks it would no doubt be easy to collect the reasons which prevented my making a reference to those papers on electricity, which had been published by Mr. De Luc previously to the period at which I wrote ; but lest any mistake should arise, allow me to state them explicitly.

In the first place, I did not conceive that Mr. De Luc had refuted Sir H. Davy's hypothesis respecting the cause of chemical affinity, although I did not think it requisite to give at that time the reasons for such an opinion, which being now called upon I have freely explained.

In the second place, I did not perceive the absolute necessity of insisting that Mr. De Luc's experiments with the Voltaic plates were from some circumstance coarsely executed. I described my own experiments, and the results; the apparatus employed, and the cautions requisite to be observed ; and I was induced to hope that Mr. De Luc and other gentlemen would repeat these experiments, and that their observations would confirm mine.

In the third place, In stating some opinions relative to the excitement of the Galvanic pile, I did not notice that they were in opposition to Mr. De Luc's conclusions, because I felt persuaded that all those gentlemen who might honour my communications with a patient perusal, would also be well acquainted with the papers of so distinguished a philosopher as Mr. De Luc ; and to their judgement I submitted the difference between our views on the subject, which difference it would be altogether impossible for them to overlook.

In the fourth place, I did not notice Mr. De Luc's experiment in which a very slight degree of excitement was perceived in consequence of friction between a glass rubber and glass cylinder, because I did not think it satisfactory in itself; and deeming it at all events an anomalous exception to a general rule, and not likely to be admitted in opposition to strong analogy. But it, as I must now do, to operate with all the force it possesses.

Having endeavoured, sir, to explain and defend, in a candid

R 3 manner,

manner, those parts of my papers which have been thought cen-
surable by Mr. De Luc, I cannot omit acknowledging the grati-
fication I have derived from his approbation of the other parts ;
and the more particularly as his principal objections to my state-
ments, those which he thinks supported by his own experiments,
appear to me to have arisen from his having misunderstood the
purport of what I have written.　I beg leave to express the very
high consideration and respect I have for him, and

　　　　　　　　I am, sir,
　　　　　　　　　　　Your obliged and faithful servant,
　　　　　　　　　　　　　　J. D. MAYCOCK, M.D.

LIII.　*Essay on Agriculture, as a Science, subdivided into se-
parate Departments.*　By W. RICHARDSON, *D.D.*

I HAVE often lamented that agriculture, far from being consi-
dered as a science, and treated as such, was reduced merely to a
measure of practice, and left in the hands of persons little quali-
fied to advance the theoretical knowledge of this useful branch
of learning, and little disposed to improve its practice, by chan-
ging the usages to which they were most obstinately attached,
or even to admit that their practices were capable of receiving
improvement.

　This earliest, and most necessary of all sciences, ought, as I
think, to be considered as consisting of three separate depart-
ments, distinct from each other; the theoretical—the experi-
mental—and the practical.

　The first and second are at present quite absorbed by the
third, without any prospect of emerging in their proper and di-
stinct characters.

　I shall endeavour to describe the qualities which I conceive
the dormant personages representing these several departments
ought to possess, and their respective offices.

　The theorist should be well acquainted with natural history
in general, as well as with that of the several vegetables we are
used to cultivate for our own consumption or that of our do-
mestic animals;—their habits, their properties—their seasons
of attaining perfection.—He watches the process of Nature with
attention, and combines his general observations with those he
has made on the particularities of each separate vegetable, and
then speculates, *a priori*, on the modes of culture best suited to
them; and the soils best adapted to them, and likely to make
them bring forward their produce in the greatest abundance and
highest perfection.

　Are the suggestions of the theorist to be immediately adopted
　　　　　　　　　　　　　　　　　　　　　　　　　　and

and carried into practice? By no means;—they must under-
go the test of experiment. Here the second department of the
agricultural school, as arranged by me, opens, and a new per-
sonage is introduced.

The experimentalist should be careful, patient, and diligent,
without prejudices or even opinions on the subjects before him :
he is to make his experiments on the very smallest scale, so that
he can diversify them without expense, and without having any
interest in their success,—failure is to him exactly the same
thing ; as information is his sole object.

This personage adopts the ideas, and if you please the whims
of the theorist, which he is not to presume to call Utopian—he
gives them a fair and patient trial under different circumstances,
and on a small scale. Shall he discover any thing, in the slightest
degree promising, he repeats, and varies his experiments, until
he satisfies himself, either that the measure is a vain one, or that
it deserves attention. In this latter case the experimentalist
makes his report to the agriculturists, recommends to them to
try the measure on a larger scale, and in actual practice.

Even expense, ultimately so important, is not in an early stage
to stop proceedings; for the object immediately before the school
is to devise by what means the vegetable in their hands can be
brought to the highest degree of perfection and utility;—the ques-
tion of expense comes next ; this on his diminutive scale, is no-
thing to the experimentalist,—but should it threaten to be
weighty, the ingenuity of all parties is now to be exerted to
find succedanea; and a knowledge of the subject being acquired,
measures may be devised which will attain the object by more
accessible means.

The third character in the drama is the practical agriculturist,
of whom I complain that he has taken upon himself the whole
three characters I mentioned: He treats the theorist with super-
cilious contempt, as presuming to obtrude his wild speculations
into a department of which he considers himself as complete
master.

Hence improvements are discouraged, and discoveries that
might have proved useful are nipped in the bud.

The second character I wish to introduce does not yet exist;
whence it comes that discoveries which have been forced into
attention rarely meet with a fair trial : they are encountered by
the practical farmer with prejudice, and even with jealousy; they
are considered as obtrusions; and treated as uninvited, unwel-
come strangers.

It is some thirty years since Dr. Lettsom brought mangel wur-
zel to England, and was strenuous in his exertions to teach the

use

use and value of that excellent root: yet the English agriculturists, though so fond of house feeding, let it slip through their fingers: and if, after a lapse of many years, they have at last become sensible of its value, it was owing to the strenuous interference of my amiable countrywoman, the marchioness of Salisbury: it is, at least, to this noble agriculturist that we owe the introduction of this important vegetable into Ireland; and I am proud of having been an active instrument under her ladyship, who was so good as for years to supply me with seed.

Sometimes, indeed, the practical farmer persuades himself that he has assumed the character of the experimentalist, and tells us he has made the experiment;—that is, he has cultivated a field in a particular way. But it is not from solitary trials on a great scale that information is to be obtained; experiments lead us to knowledge by comparison,—they should be multiplied and diversified.

Hence agriculture, as a science, is at a stand;—the present possessor of the field, perfectly satisfied with his own attainments, and in high admiration of his own practices (often very good), does not admit improvement to be necessary, and indignantly rejects any innovation.

He is encouraged in his contempt for theoretical speculations, by the ridicule which a witty author throws on the agricultural projectors of his day.

It is just a century since Swift made a bitter attack on the Royal Society, which he describes *as a set of projectors, lately incorporated by royal patent.*

It is not for me to defend this respectable body: a century has intervened since this wanton attack was made upon them, and their merits or demerits are best appreciated by their intermediate proceedings and transactions.

My object in referring to the passage in Swift's Laputa, is to throw light on the arrangement I have made in the agricultural science, and to afford proof of its propriety.

Swift says, " the professors contrive new rules and methods of agriculture—new instruments and tools; all the fruits of the earth shall come to maturity, at whatever season we think fit to choose, and increase an hundred-fold more than they do at present."

He states " the result of all this to be, that none of these projects are yet brought to perfection, and in the meantime the whole country lies miserably waste; by all which, instead of being discouraged, they are fifty times more bent on prosecuting their schemes."

Admitting this to be a fair account of the facts in Swift's day,
(which

(which I much doubt,) the picture he draws is a necessary result of *his own* statements, from which we can infer,

That in his time projectors were wild 'and speculative—practical agriculturists not quite so averse from innovations as at present, but equally tenacious of their practices when once adopted.

The whole mischief (admitting it to have existed) obviously arose from Swift's having omitted a personage in the agricultural drama, forming a coalition between the wild theorist and positive practical farmer; omitting the intermediate personage, the experimentalist, who would have protected them both from mischief, suppressing the extravagancies of the projector, and paying every attention to his suggestions that bore the test of experiment; and suffering nothing to pass into practice, which did not afford a reasonable prospect of advancing the agricultural science, and multiplying the benefits derived from it.

Let us try two or three agricultural questions by the test of the arrangement I have suggested, and we shall see what progress the science has made without them, and to what state it probably would have advanced, had they been adopted.

I commence with the gramina, a branch of agriculture to which for twenty years I have paid considerable attention, and which, for these last ten, I have considered as my peculiar department.

The great importance to us of grassy produce is obvious; and nature has been very liberal to us in that line—she has given us (as botanists tell us) one hundred and fifty varieties to supply the wants of our cattle, and to exercise our ingenuity in discovering their uses and developing their properties.

What use has the agriculturist derived from this copious stock? But a solitary one.—He has discovered that rye grass when sown with clover makes an excellent mixture; into further practice his knowledge does not carry him: and yet in dogged confidence he turns a deaf ear to any suggestions for increasing his stock of grasses, or advancing his knowledge on their subject.

What would probably have been the result, had agriculture been distributed into the three departments I have supposed, and the gramina had passed through the hands of the theorist and experimentalist, before they reached the practical farmer?

The theorist, speculating *a priori*, would have considered what were the properties most likely to give value to grass, and by which it would be made most useful to our cattle: he would soon have perceived that three were prominent—earliness—luxuriance—and quick powers of reproduction after being cut or eaten down; he would have desired the experimentalist to make many small plots, and to compare the different grasses in these several points of view.

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The experimentalist would soon have discovered that the grasses possessing all, or the majority of these valuable qualities, were very few in number, and that rye-grass was not among them.

When I cease to generalize, I shall give a small essay on the grasses worth cultivation, having ascertained their comparative values, after much pains, and with great accuracy.

The miserable ignorance of the practical agriculturists on the subject of the gramina is easily accounted for; they have consigned the task of instructing them on all rural subjects to two descriptions of persons—the Grub-street writers, who, without ever having cultivated or perhaps seen a farm, maintain themselves by furnishing agricultural magazines and newspapers with essays on such subjects as their employers point out, conceiving them to be popular at the time.

The second description of public instructors in the agricultural line are, the seed- and nursery-men, who, having their goods to dispose of, take the opportunity of displaying their own great knowledge, and of puffing their saleable commodities; these gentry often publish agricultural volumes, which they find a very lucrative trade, as the English buy every thing in that line, conceiving they are purchasing information.

The ignorance of these charlatans is scarcely credible, nor is it easy to tell to what mischief it may have led; both the seedsmen and the books of their predecessors have recipes, nostrums, stating mixtures of eight or nine different species of grass, which they advise agriculturists to throw together in certain proportions of the seeds, and then to sow one mixture for meadow, another for pasture.

An examination of these lists by any one who understands the natural history and qualities of the several grasses, will instantly discover the mischievous ignorance of these quacks.

I should not have spoken so boldly on this subject, had I not evidence under the hands of the first seeds-men in London to confirm what I say.—It is common for improving gentlemen to desire their seeds-men to send them such a mixture of grass seed as will suit their soil. The order is instantly complied with, and the list established by the bill. I answer for it, whoever consults his bill for this list, will find many worthless grasses, of incompatible periods; and that he will find tall oat grass, *avena elatior*, recommended, and sent to him.

Now tall oat grass is by far the most mischievous of the squitch tribe; it has small bulbous roots like the crocus, and is known in this country by the names of purl grass and knot squitch; it is reprobated by our farmers as one of our most troublesome weeds.

Such is the style of instruction we receive from the present
<div align="right">agricultural</div>

agricultural authorities; and so long as that task is left in such unhappy hands, it is no wonder that our knowledge of any particular branch of this useful science should be sadly limited; or that Sir Humphry Davy should complain, that our acquaintance with the gramina should be confined to two species*.

Better prospects are now opening; a revolution has fortunately taken place;—philosophy supported by science has stepped forward, and the Board of Agriculture, with its able chemical lecturer, have rescued the gramina at least from the hands of mercenary ignorance.

This unexpected irruption of science into the peaceful and productive domain of these indefatigable scribblers has excited no small uneasiness in Grub-street; one of the gentlemen, whose name is perpetually occurring in almost every agricultural publication, shows the alarm that school has taken, at the dangerous invasion of their territories. He says:

" 1 am far from wishing to depreciate the use of science, as directed to the improvement of agriculture; but the pleasure, the delight there is in studying Nature through these spectacles, is greater than its benefit."

" All that science has done to improve our knowledge of the value of the grasses as yet, consists in showing which affords the greater proportion of saccharum:—with those who sing the praises of analysis, the burthen of the song is saccharum."

" Can it be reasonably said that the choice of rye-grass, as a separate grass, was the result of accident? This text that is quoted from Sir Humphry Davy is not Gospel." Again.

" This account is miserable, because it is not true; and the hasty expressions of great men ought to be more carefully repressed, as they are the more widely diffused."

I know not any question in rural practice that more requires the interference of the scientific theorist than the proper period for mowing, nor any point upon which the practical farmer is more ignorant or more opinionated—he prides himself on having saved this hay before others, and boasts of its fragrance and tea-like verdure.

The theorist, acquainted with natural history, would have told him that the juices of all vegetables attain their greatest per-

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* Sir Humphry Davy admits two varieties to be in use; but the second, cocksfoot, is a recent introduction; and the first recommendation of this luxuriant grass to the practical farmer, will be found in the Transactions of the Royal Irish Academy, six or seven years ago, in a memoir of mine, on the useful grasses, with my reasons for strongly recommending cocksfoot, deduced from its natural history.

Whoever has published any earlier recommendation of this grass to the agriculturists of his country, is entitled to the credit of its introduction.

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fection in their inflorescence—that it is at this period alone all
extracts from vegetable substances are taken : and as in the
case of hay the whole vegetable is preserved, it is of great im-
portance that it should be mowed in its highest state of perfection,
that is, when the predominant varieties of grass are in flower.

The practical farmer knows nothing of all this : he has his own
rules for deciding on maturity, and generally cuts his crop be-
fore either the cocksfoot or the rye-grass (the two earliest of
our predominant grasses) are in flower.

I sometimes feel an ill-natured pleasure when I see the tramp-
cocks of these early gentry collapse considerably for want of
substance, giving evidence of premature mowing, and establish-
ing the inferiority of the hay.

Here the experimentalist would be useful, by enabling us to
compare portions of hay from the same crop, cut at different
periods—even the farmer himself, would he condescend to doubt,
might soon satisfy himself: by leaving the amount of a tramp-
cock uncut for one, two, or perhaps three weeks, later than the
rest, he would probably find his hay firmer and better; he is cer-
tain also, the quantity is somewhat increased.

Was the arrangement I recommend adopted, many agricultural
questions of much importance would receive speedy solutions.—
That of the proper seasons for sowing our several grains has
been much agitated.

Upon this question the theorist would pronounce generally,
that agricultural policy directed the season for sowing each ve-
getable, to be so chosen, that it might remain above ground in
the very best portion of the year, neither exposed unnecessarily
to late frosts in its tender state, nor to premature winter seve-
rities when ripening its seed.

Hence the season for sowing each vegetable should be deter-
mined by the interval between the seed and the sickle, which
Nature has assigned to each species, corresponding with the
period of gestation in animals, and unalterably fixed at the time
of their original formation.   Upon this principle it is obvious
that the vegetables of slowest growth should be sown first, while
those of quicker progress should be delayed longer.

The question has now reached the experimentalist, who will
probably sow many varieties in distinct plots, on the same day ;
and by accurately observing their times of ripening will make
himself acquainted with their respective periods.

What I recommend here as experiment, is the actual practice
in Egypt, where they sow all their grains, of whatever species, on
the same day, that is, the first moment the retreat of the Nile
gives them access to their land, just relieved from its annual in-
undation.

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We have Scriptural authority for the result, marking the progress each separate grain had made in the same time.

Moses tells us that at the time of a particular event, " the barley was in the ear, and the flax was bolled, but the wheat and the rye were not grown up."

The experimentalist will now diversify his trials; and by sowing the same grain at different times, in many small plots, he will soon be able to determine, how far, for the security of the young tendril, he can delay sowing, without throwing the mature plant into a season unfit for ripening its seed.

It has been made a question—Whether in choosing our corn for seed, we should choose our weightiest pickle, or whether the smaller and lighter might not answer just as well ;—in other words : From which side of our winnowing heap are we to take our seed—the windward—or the leeward ? The fuller, plumper and larger grain, will not cover so much ground as the smaller, and is also of higher price; hence by sowing the smaller and lighter grain, we should save considerably; and Sir Joseph Banks is of opinion we may safely take our seed from the leeward side of the heap.

Was the question brought before the agricultural school, arranged as I suppose ;—the theorist would tell us that the farina constitutes the whole value of the corn ; that this portion of the vegetable forms no part of the organic construction, has no connexion with the vital principle of the germ, but is merely a mass of unorganized matter, provided by Nature for the sustenance of the nascent plant, until by its roots it can extract food for itself ;—that the farina in vegetables corresponds with the yolk of the egg in oviparous animals.

Now we observe that in every thing connected with the preservation of species, Nature is not only liberal, but generally profuse, and (no doubt to provide against difficulties) often redundant—besides, the provision was made when the vegetable tribe was left to propagate itself, without any of the facilities devised by man, which he now gives to assist vegetation and increase produce.

More farina, it is obvious, would be required under the hardships of a state of nature ; and a greater quantity will be formed under cultivation, as animals fostered by man acquire a degree of obesity which they never reach in a state of nature. Thus it appears, the quantity of farina is increased, and the expenditure of it diminished ; of course it is highly probable, we may with safety avail ourselves of the redundance ; that is, sow the lighter, and consume the weightier grain.

The question is now brought before the experimentalist, and one of the lightest he has to encounter ; he need only sow a few small

small plots with seeds taken from the opposite sides of the win-nowing heap, and by a careful comparative view of the crops when ripe, he will be able to pronounce upon the safety of the measure, and by attention he will soon discover what he will gain by pursuing it.

The preservation of the vigour of our soils, and the repara-tion of the waste they sustain by our perpetual call upon them for crops, and consequent loosening of their texture, by over frequent cultivation, is a subject of vast importance, and has al-ready excited much attention.

The mechanical mode is simple; to renovate, and consolidate our harassed and open soil, by mixtures of firmer materials; that is, compost formed of strong earth, or pure clay well atte-nuated: but in loose, light and sandy ground, such consolida-ting materials are rarely found: the agriculturist is therefore thrown upon his own ingenuity; and I know not any instance in which it has been more successfully exerted.

He has found, that by alternating what are known to be ex-hausting crops, with those that are deemed to be meliorating—culmiferous with root crops—farinaceous with green crops—he has brought his ground to bear more constant pressure than it was supposed capable of sustaining:—still the exhaustion, though much abated, is evidently perceivable, and the Norfolk farmers complain their grounds are tiring of their favourite turnip.

Mr. Gregg, now become very eminent as a practical agricul-turist, admits rest to be indispensably necessary, and recom-mends two successive crops of grass.

To make that rest as effective as possible, let us speculate *a priori*—Which are the grass crops that exhaust the ground least? Which are those that will consolidate and renovate it most ef-fectually? And which, during the period of rest, will yield the greatest produce?

As the question is now brought within my own immediate de-partment; when I cease to generalize, I shall on my return to the gramina, point out those which I conceive best suited to these purposes, with my reasons, and shall then leave the ques-tion in the hands of the experimentalist.

It is in adversity, when the vegetables he is cultivating are attacked by various disorders, that the agriculturist will find the benefit of the arrangement I have suggested; as it will enable him to meet with strength, and I may say, discipline, the diffi-culties he will have to encounter.

But this subject must be reserved for another letter.

Clonfecle, Moy, May 31, 1816.   W. RICHARDSON, D.D.

LIV. *Ex-*

LIV. *Extract from a Memoir on the Combinations of Phosphorus with Oxygen.*

Read to the French Academy of Sciences on the 1st *and* 15th *of July* 1816. *By M.* Dulong[*].

THE chief object of this paper is to prove that there are at least four distinct acids, formed by the combination of phosphorus with oxygen. The acid with the minimum of oxygen, which I intend to call *hypophosphorous* acid, is produced by the reaction of water on the alkaline phosphurets. When the latter are properly prepared, there results from the decomposition which they make the water undergo, phosphorated hydrogen gas in variable proportions, and two acids which neutralize precisely the base of the phosphuret. One of these acids is the phosphoric acid, and the other is the hypophosphorous acid. By employing the phosphuret of barytes we may obtain very easily the latter acid in its state of purity. For this purpose it is sufficient to separate by the filter the insoluble phosphate from the water which holds in solution the hypophosphite of barytes, and to precipitate the base of this salt by an adequate quantity of sulphuric acid. The acid solution which remains may be concentrated by evaporation ;—pure water only is extricated, and we obtain a viscous liquid strongly acid and uncrystallizable. By a stronger heat we decompose it : phosphuretted hydrogen gas is developed, a little phosphorus is sublimed, and phosphoric acid remains in the retort, partly combined with the glass. The hypophosphorous acid acts, in general, as a very energetic deoxidant.

The hypophosphites are remarkable by their extreme solubility. None are insoluble; those of barytes and of strontian even crystallize with great difficulty; those of potash, soda, and ammonia are soluble, in all proportions, in highly rectified alcohol. That of potash is much more deliquescent than the muriate of lime : they absorb slowly the oxygen of the air, and become acid: they are decomposed by the action of heat, giving the same products with the hypophosphorous acid.

We cannot effect the analysis of this acid by direct means, since none of its combinations can be obtained in the dry state. In order to ascertain its proportions, I transformed an indeterminate quantity of it into phosphoric acid by means of chlore. The quantity of chlore employed to produce this effect, and the weight of the phosphoric acid which results from it being known, as well as the proportions of the latter acid, it is evident we have all that is wanted to resolve the question. This analysis being

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[*] *Annales de Chimie et de Physique*, June 1816, p. 111.

very complex, and resting on a great number of data deduced from experiments, we cannot expect from it a perfect exactitude. I found by this process, that the hypophosphorous acid was formed of 100 phosphorus, and 36·3 of oxygen: but according to the composition of the phosphoric acid, which may be known with the utmost precision, the number 37·44, which is the three-tenths of the oxygen contained in this acid, seems to be nearer the truth. According to this, the hypophosphorous acid must be composed of:

Phosphorus .. 72·75—100
Oxygen .. 27·25—37·44.

$$\overline{100}$$

These results are calculated upon the hypothesis that the hypophosphorous acid is a binary combination; but we may entertain doubts on this method of regarding its nature, and there are even strong reasons for believing that it is a triple compound of oxygen, hydrogen, and phosphorus, forming a new species of hydracid. This is a question which I shall, by and by, attempt to resolve.

The acid which is immediately above the latter, results from the decomposition of the chlorure of phosphorus at the minimum, by water: it is to Sir H. Davy that we are indebted for this discovery. We obtain this acid perfectly pure by evaporating in a proper manner the water in which the decomposition of the chlorure has been effected. The whole of the muriatic acid is disengaged, and, upon cooling, the acid crystallizes. It seems proper to preserve to this substance the name of phosphorous acid, which has been hitherto given to the product, from the slow combustion of phosphorus, the nature of which, as we shall soon see, does not agree with such a denomination.

The true phosphites have not yet been described: their solubility is in general much less than that of the hydrophosphites. The phosphite of potash is nevertheless very deliquescent, uncrystallizable, but insoluble in alcohol. Those of soda and of ammonia are also very soluble in water. The former crystallizes in rhomboids approaching to the cube. All the rest are little soluble in water: those of barytes, strontian, and lime crystallize by spontaneous evaporation; but if we wish to concentrate their solutions by heat, a division is made in the elements of the salt; a precipitate is formed composed of small crystals, similar to the acetate of mercury:—these are salts with excess of base, absolutely insoluble in water. There remains in solution a salt with excess of acid, which crystallizes with more difficulty. Thus there exist surphosphites, subphosphites, and neutral phosphites.

The phænomena presented by the calcination of the phosphites

phites are nearly the same with those of which we have spoken in treating of the hypophosphites.

I have analysed phosphorous acid, of which Sir H. Davy had already given the proportions, to ascertain how much of chlore the phosphorus absorbs in order to pass to the state of chlorure at the minimum. My results do not differ sensibly from his. I have found by this means that the phosphorous acid is formed of   Phosphorus   ..   57·18—100

                 Oxygen   ..   ..   42·82—74·88

                                   100

Hence it follows that the oxygen of the hypophosphorous acid is to that of the phosphorous acid as 1 : 2.

The acid produced by the slow combustion of the phosphorus, in the air, to which several phænomena may be referred, is nevertheless very little known. It differs from the foregoing not only by its proportions, but also by its nature. It is not combined in the way it is with the oxides. The salts which have been described heretofore by the appellation of phosphites are not peculiar salts; they are either phosphates, or more frequently a mixture of phosphates and phosphites.

Ought we to consider this substance as a simple mixture of phosphoric and phosphorous acids? Sir H. Davy merely makes this assertion, but without any proofs. I do not think, however, that we can admit this idea ;—for, why should the conversion of the phosphorous acid into phosphoric acid stop at a certain point of time?—wherefore should we find constantly in this substance the same proportions of oxygen and phosphorus? If its formation took place in a rapid manner, we might conclude that some particles of phosphorous acid escaped the combustion; but the tediousness of the process of obtaining it excludes all idea of an incomplete combination.

We might also suppose that the oxygen of this acid, forming a binary combination with the phosphorus, is unequally divided between two parts of the radical by the action of the bases, and that there would result from this division phosphorous acid and phosphoric acid: but it seems much more probable that these acids, even before the action of the oxides, are already completely formed and combined with each other, like the elements of a salt. It is by adopting this last opinion that I intend to call it *phosphatic acid*; an appellation which reminds us that this acid has some analogy with the phosphates in its mode of composition.

M. Thenard has found by direct means that the phosphatic acid ought to be formed of 100 parts of phosphorus and 110·4 oxygen. I obtained 109 by a different method. Neither of

these numbers is found in a simple ratio with that of the oxygen of the phosphoric acid. The simple ratio which they approach the nearest is that of 9 : 10, which will suppose 112·4 of oxygen in the phosphatic acid. Although the difference between the calculation and the observation is as much as two centiemes, I think it very probable, for other reasons about to be explained, that these two acids are really in this proportion; and this is what I purpose, besides, to verify directly in another manner.

It must be observed on this subject, that when, in a series of binary compounds formed by the same elements, there are two very much alike, that which is not in a simple relation with the others ought to be considered as a combination of two more simple compounds. It is absolutely indispensable to admit this idea, if we wish to preserve the theory of chemical proportions in all its simplicity: and this is happily what experience confirms in an evident manner. It is thus, for example, that if we wished to regard the three oxides of iron as primary combinations, we must admit at least six molecules of oxygen in the inferior oxide, and at least nine in the oxide at the maximum; for the quantities of oxygen contained in these three oxides are as the numbers 6, 8, and 9 : but admitting that the intermediary oxide is formed of two molecules of red oxide and one molecule of oxide at the minimum, we are only obliged to suppose that there are two molecules of oxygen in the oxide at the minimum, and three in the oxide at the maximum. This ceases to be a pure supposition when we observe the division into red oxide and protoxide, which the deutoxide of iron presents in almost all experiments [*].

The exact determination of the proportions of the phosphoric acid was indispensable for the analysis of the preceding acids; and the discordance of the results obtained hitherto by chemists equally expert, imposed upon me the necessity of searching for the causes of error which might exist in the processes resorted to, and to employ others which should be beyond all suspicion.

I examined in the first place the process of the acidification of phosphorus by the nitric acid, and I saw that it did not merit

[*] The second number of the Journal of the Royal Institution of London, which did not appear until after my paper was read, contains an extract from, or rather a severe criticism on, a work written in Swedish by M. Berzelius, in which we find an explanation similar to that which I give here. It is a great satisfaction to me to have coincided with a man of talent so distinguished. Although I regard this opinion as very probable, I do not pretend however that it cannot be combated. But it is not with the ironical, injurious and offensive tone which reigns in the whole of the article just mentioned, that we ought to attack labours the whole object of which is an inquiry after truth. If this kind of criticism is once introduced into the sciences, their progress will be greatly retarded.

any confidence. We obtain results much more constant by substituting for the pure phosphorus in the same experiment a metallic phosphuret, the proportions of which may be determined very exactly by synthesis, when it is prepared in the way I have described. By acidifying the phosphorus by the action of the chlore in contact with the water, and determining the quantity of chlore employed for this purpose, we may also attain very satisfactory results. Finally, on examining by synthesis the proportions of the chlorure of phosphorus at the maximum which corresponds with the phosphoric acid, we may attain very great precision. Sir H. Davy had already employed this last method. Our results were so different, that I at first suspected there was some error on my part; but having constantly obtained the same numbers, I regard the following proportions as very near the truth. Chlorure at the maximum:

Phosphorus	..	..	15·4—100
Chlore	..	..	84·6—549·1
			100

Hence phosphoric acid :

Phosphorus	..	..	44·48—100
Oxygen	..	..	55·52—124·8
			100

On comparing the analysis of the phosphorous acid which we have given above, with that of the phosphoric acid, we see that the quantities of oxygen in these two acids are in the ratio of 3 : 5, instead of 1 : 2 as Sir H. Davy has indicated.

According to the series of the combinations of the phosphorus with the oxygen, we are well founded in admitting that the phosphoric acid is formed of two atoms of phosphorus and five atoms of oxygen. On this supposition, and representing the oxygen by 10, the relative weight of the atom of phosphorus will be 20·03; that of the phosphoric acid = 90·06, &c.

I also directed my attention, and even during a very long time, to the analysis of the phosphates, in order to discover the laws of composition of those salts. M. Berzelius, from the analysis of two phosphates only, has concluded that the oxygen of the acid is double that of the base; but the salts which he has examined are certainly not neutral salts. I have analysed a great number of phosphates, and I am not yet able to explain all the variations which I met with in some species. I adhered to the composition of the phosphates, in order to discover that of the phosphites and hypophosphites ; the comparison of the proportions of those different salts being very interesting in point of,

theory.

theory. This labour not being yet terminated, I shall confine myself for the present to saying,

1. That the neutral phosphites are changed into phosphates without ceasing to be neutral, as M. Gay-Lussac had already observed.

2. That the neutral hypophosphites become acid phosphates.

3. That the phosphoric acid strongly calcined contains a quantity of water, the oxygen of which is as the third part as is the case in some phosphates.

4. That the metallic phosphurets obtained by the process which I have indicated, correspond with the protoxides soluble in the acids; that by passing the phosphorus to the state of phosphoric acid, and the metal to the state of protoxide, there results a neutral phosphate, in which the oxygen of the acid is to the oxygen of the base :: 5 : 2 ; and consequently, if the metal passes to a higher degree of oxidation, there is formed a sub-phosphate, in which the ratio of the quantities of oxygen becomes that of 5 : 3 or 5 : 4.

5. That the phosphites and the phosphates have, with the nitrites and nitrates, a very great analogy as to proportions;—that the same analogy has been already remarked in the proportions of the acids with a base of phosphorus and azote.

6. That sulphur and phosphorus do not present so many points of contact in their properties as generally supposed.

7. That the forces which produce the combinations seem to flow from another source than those which determine their proportions.

8. Finally: that when one and the same body can form several acids with oxygen, the same base produces, with these acids, salts so much the more soluble the less oxygen there is in the acid.

LV. *On the Mosaic Cosmogony.* By *Mr. A. Horn.*

To Mr. Tilloch.

SIR, — NEVER having been a principal in the dispute respecting the Mosaic Cosmogony,—though it is now terminated, may I be allowed, as an " auxiliary," to make one or two remarks upon the subject? I wish the more especially to do so, because the party that has ostensibly left the field, retires as if his positions never have nor ever can be refuted.

On reviewing the controversy, the anti-cosmologist may be compared to a foreigner, who presumes to decide upon the merits of our great dramatic poet by a French translation; and

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though repeatedly corrected, and reminded of his injustice to the author, persists in urging his *vernacular proofs*, that Shakespear is a mere playwright, destitute of genius and truth in every plot and character. Had the anti-cosmologist understood but a little of the language in which the cosmogony is written, the pages of the Philosophical Magazine never would have been the record of the following unphilosophical conclusion : ' Neither forest trees, shrubs, nor lichens come under the description of *grass, seed-bearing herbs, or fruit-trees.*' In this reflection he relies upon the common version, which makes רשא a generic noun, contrary to the analogy of the Genesis: the terms that characterize organized beings, except where the rational species is designated, are *all general.* The eleventh verse literally runs thus : ' God said, Let the earth *shoot forth the vegetable* (אשדח ץראה אשד), the herb making seed, the tree producing fruit for its kind, whose seed is in itself, upon the earth.' The objection that, if this account were fact, we should have no *forest*-trees, is extremely vulgar ; as if the *oak* and the *beech* came not under the denomination of ' fruit-bearing trees,' merely because their *mast* and *acorns* are never found among the *nuts* and *oranges, grapes* and *nectarines,* that decorate the *dessert*-table.

The inference, so pertinaciously defended,—that Moses has confined the existence of the aquatic animals to the *fifth* day,—is equally unfortunate. Moses informs us that, at the commencement of this period, ' God said, Let the waters bring forth *abundantly* the moving creature that hath life.' Gen. chap. i. ver. 20. The author of the Cosmogony never designed here to be understood as if no species of animated beings had before existed. His language is very different from that in which he describes the first production of land animals.—We have the authority of Moses himself, in another instance, for the import of his language in the present case.

In the following passage (Exod. chap. viii. ver. 3.) the mode of expression is here precisely the same with that above mentioned in the Genesis : ' The river shall bring forth frogs *abundantly.*' Would it not be a monstrous absurdity to argue from this passage that, because Moses says nothing of their previous existence, therefore there must have been no frogs in the Nile before this event ? though it would be very difficult perhaps to prove the fact. But as there certainly were frogs in Egypt before this period ; so, if the same words have the same meaning, there were ' living creatures' in the waters previous to the *fifth* day; though till then they did not so *abundantly* exist. Besides, a physical cause can be assigned for the peculiar expression Moses uses, which also contains a sufficient reason for the aquatic animals remaining unnoticed till this period. The waters had hitherto been

been so highly impregnated with the earths, that stratum *super stratum* were so rapidly deposited that they proved the grave rather than permanent habitations for their tenants. Hence many of the species perished entirely, and others were vastly diminished. But in the fifth period the water, being sufficiently purified by immense depositions, not only permitted the more perfect orders of aquatic animals to exist, but the Creator replenished the ocean with an increased number of inferior inhabitants suited to the improved state of their element.

<div style="text-align:center">I am, sir,</div>

<div style="text-align:center">Your very obedient servant,</div>

Wycombe, Oct. 9, 1816. <div style="text-align:right">ANDREW HORN.</div>

LVI. *A new View of Vegetable Life.* By *Mrs.* AGNES IBBETSON.

To Mr. Tilloch.

SIR, — THE very curious fact I have now to exhibit throws (I think) a new light on vegetable life, perfectly *confirms* the truth of all I have hitherto shown of the history of their general formation, and renders the commencement conformable to those *universal laws* which appear to be established not only in the vegetable, but in the animal and mineral world; as I shall show at the conclusion of this letter.

In my last I depicted the curious manner in which the flowers are developed in the interior, both in trees and herbaceous plants, the year they are *completed* at the exterior of the vegetable. But I am now authorized to suppose that there is a *prior formation*, both of leaves and flowers, prior to that in which they are collected and enlarged in the middle of the plant. In the first I was able to follow the formation in a *regular series*, till they appeared opening into flower, and making their way out of the vegetable *through the buds* in the usual manner; but the *buds* in this case are really the *vehicles for completing* and sending out the bunches of flowers and leaves, and *not* (as it was supposed) the part in which they are formed:—yet to trace in a series what I am now going to show, *is impossible,* as it will not admit of it. I shall, however, give an exact account of the whole process, and the consequences that must (I think) result from such a formation.

Having cut an extremely thin specimen of the wood of the *cupressus* longitudinally, and placed it in my slider, *under* my best microscope (though using very low powers) I was surprised to see a sort of running pattern of *leaves* and *flowers* adorning every two or three stripes of the wood, and now and then collecting

<div style="text-align:right">lecting</div>

lecting in thicker patterns, when the lancet, in passing over the raised part, pushed them up together, aggregating their numbers. See fig. 1, Pl. No. 3. [Plate IV.]

After examining many different cuttings, I got a fresh branch of the elm, the beech, the oak, the plane, and rose, and cut specimens from each :—still the same object was visible, though arranged not exactly in the same manner, yet in stripes of flowers. Convinced I was not mistaken, I made a bold effort at proving the fact to others, assured that if Sir William Herschel (who happened to be here) saw it, no one would hazard the contradicting *a sight* whose perfection was so *well acknowledged*, or a *judgement* so well *matured*, though they constantly disputed mine. I requested him, therefore, to prove the fact : and with the generosity and good-nature for which he is so much loved, he came and *examined it* directly ; and said, *he saw it most plainly*. I then showed him an horizontal piece, in which he also saw the same appearance of leaves and flowers (see fig. 6). Assured therefore it was a truth ; the first specimen was allowed to rest for several days, while I was examining *herbaceous plants :* but great was my astonishment when I next viewed it, to find that it had *thrown off* many white and *double lines*, between which flowers and leaves did appear to be formed, and that in many places complete bouquets had also aggregated of a glutinous jelly-like matter;—in short, that vegetation seemed to have continued and prolonged itself even on the glass ; for as the wood had been cut with a pair of scissars, making its edge exactly even, each addition would be most visible. This was indeed a fact worth ascertaining; and after trying it repeatedly, I again requested the favour of Sir William Herschel's assistance. He gave it me with the same simplicity and kindness as before—directed me to divide the talc into squares, thus regularly magnified, that I might be sure of the increase of the part ; and then taking an exact drawing of *the specimen* by that measurement, there could be no fear of my not knowing the real quantity *added, even to a hair's breadth*. This was completed on Friday; but on Sunday morning just *looking at it*, I was not a little vexed to see that (great damps having prevailed in the atmosphere) much moisture had insinuated itself between the two talcs ; and would, I was fearful (before Sir William Herschel came on Monday), destroy that *clearness* and *distinctness* of form for which both leaves and flowers are *remarkable*. I therefore prepared two other specimens ruled in the *same manner*. In three days, though the wet had, from a renewal of moist weather, still in some measure injured it, yet the lines and flowers were perfectly delineated :—indeed, the only harm mois-

ture does, is to render each individual part *less distinct :* still, however, they were sufficiently so, to be perfectly perceptible to Sir William Herschel, who said he thought there could be *no doubt* that it was the *continuation of vegetation on the glass.* This was indeed a great matter gained, especially when *so witnessed.* Sir William Herschel also did me the honour to see the first specimen.

Fig. 2 was the spoiled piece, which had however increased to fig. 3. Fig. 4 was one of the specimens Sir William examined, which was increased in three days to fig. 5; and another which I have not added, fearful of augmenting *too much* the number of specimens. Indeed the collection of flowers on the glass was evidence sufficient of the fact; for no power but their own could place them *there.* The white specimen might be, and *was laid on the glass;* but the rest could not be either *taken off* or put on, unhurt, without Nature's help.

On further examination I found that, if I *watched with great exactness,* drops of moisture were to be seen ejected from the many cut ends of the line of life; and that when the more diminutive lines of that part increased in length, wherever the liquid had *fallen,* and the double line also *passed through it,* bunches of flowers had protruded, till they became either wreaths or bouquets, running and growing in a fanciful manner as long as the moisture would sustain or *assist in forming them;* sometimes even more than a week. It could no longer be a doubtful fact; for on examining the *talcs* and glasses I found them covered with different beautiful patterns, which had proceeded from all the specimens accidentally thrown on the surface; and so opposite is the jelly-like appearance of the new propagation, to the white one presented to examination, that it is not possible to mistake them, or to take one for the other: the lines, indeed, are always white, but the *flowers* and *leaves,* I find, require more than a fortnight to become so: and such is the glutinous power with which they stick to the glass, that nothing but soap and sand can eradicate them; nay, even a knife is sometimes required to scrape the wreaths off, as they absolutely almost indent the talc: yet so high do they lie above the surface, from the quantity of leaves and flowers in the bouquets, that they are easily injured. But great care should be taken that a confusion is not made between the white lines of the increasing vegetation and those that are formed by cracking the talc, for I have now discovered that they are very much alike : however, it is the lines only. It is better therefore to take it on glass. My glasses are absolutely *covered with beautiful patterns,* and the specimens will not only increase from the ends, but some will

augment

augment their bouquets, both above and under (if not fixed too close to the glass), wherever moisture has prevailed; but I have never seen any part of the wood *grow*; it is merely the leaves and flowers, with the double line, which is certainly a diminutive thread of the line of life. That this matter on the glass is a *continuation of the vegetable life,* and fig. 1. a prior formation of the preceding year, *I cannot doubt.* I believe also it is merely the corolla, *pericarp,* and pistil, that carry the appearance of a flower in the stripes. I have generally found

them *round, oval,* or some form like this: but

so extremely diminutive that it is impossible the flower should (I think) contain any thing more than the parts I have named, and perhaps the *stamen cases;* and if it is a prior formation protruded the preceding year, it is reason enough to account for the different ingredients of the flower being all made in a separate place;—for, being composed of such various and dissimilar parts, might not the juices intended to form the pistil be highly detrimental to the pollen? and thus with the rest. Hence probably the extreme pains Nature takes to divide the various liquids necessary to vegetable life, and the uncommon arts *resorted* to to prevent the possibility of their meeting, by confining them within layers of cylinders without any means of communication. This law is so evident and universal, that it is generally the first idea with which a dissector is struck, as it is most plain and positive in every plant. Du Hamel observed it, and has given *a print of it;* as well as Du Petit Thouars.

If, therefore, we suppose this part of the flower presented in fig. 1. to be a prior formation, it is most probable that it should be *protruded* in a *separate state,* from the pollen and seeds. I have accordingly laid it down in this manner.

The corolla, pericarp, and pistil, formed the first year, and followed the next by the insertion of the heart of the seeds and the pollen; which when so far ready rise up from the root in a different part of the stem, and after various preparations, regularly to be traced from point to point, enter the pericarp, where the formation of the flower is complete, and where the whole process terminates, by the displaying their scents and beauties in the open air.

The *flower-bud* and *leaf-bud* therefore must be only intended for the receptacle of the unfinished flowers and leaves, the place in which those parts are completed, and acquire the last finish to their respective forms. The *flower-bud,* when at last fixed in its proper place, is followed by the flowers which were formed

the

the preceding year, now entering the flower-bud for the first time to complete their forms; by the *heart* of the *seeds* when ready, drawing themselves into the *pericarp* by means of the line of life, with which they are tied; and the pollen powder into the stamen. The whole then closing, till near the time of fructification; when the enlarged flowers requiring new stalks (again leaving the flower-bud which falls off, being scales only) the flowers shoot up into perfection;—then the seeds fill up their heart, acquire their outward part; and when the flower dies, they soon complete their cover, and drop into the earth. The *leaf-bud* also, by taking in the leaves, and completing their edges and upper cuticle (being evidently an after process), and thus gave the idea of their being formed *there:* nay, I have reason to think their pabulum is also a late formation, the bark-juice being then undoubtedly propelled into the leaf-bud.

Thus this new discovery only more completely establishes all the facts I have hitherto made known of the formation of plants, and confirms some of the propositions I wished to prove; viz. that the seeds, pollen, and *flower-bud*, are formed in the root: it shows that also the *flower-bud* and *leaf-bud* are a totally different substance from the *flower* and *leaf*; and that the account I have before given of the passage of the flower-bud through the wood is a *real fact*, and which this new discovery by no means invalidates. Indeed, it accounts for some trifling contradictions that did not quite *assimilate* with the general plan, and for some difficulties I could not *before understand*. The first was the earliest separation of the *flower-bud* and flower, when *in the root*, as every author and every prejudice had taught me to seek the flower in the *flower-bud*. Now this prior formation completely clears the whole, and shows that the flower-bud was merely intended as a case to perfect the flower, not to commence it. How beautiful are they now made to meet, and the flower to enter there for the reception of its various ingredients! In the second place, it showed that what *I took* for the first *commencement* of the leaf was the *weaving* of the scales, and the finishing the leaf: indeed, I never before could account for the little branches of *extremely diminutive leaves* I found also in the leaf-bud, with the loose fibres weaving other parts: the latter were for the *scales*, the former the real *unfinished leaves*: while the leaf-bud is certainly formed in the *bark*, as I have before shown. Thus, though the whole has been discovered by *detached pieces*, it all arranges itself in a perfect regular manner, and, like a *cut map*, each part fits into the place appropriated for it. If this does not prove it true, I know not what will, since perfect consistency belongs to God alone: and I feel thoroughly conscious of my inability to produce or invent such a plan. In short, every

additional

additional discovery, and evey new view we take, *prove*, that of the form and proceedings of vegetable life we before knew *nothing;* all our *facts* have *proved false;* all our conjectures *mistakes*; and that we must begin by banishing every former idea that *respects the real nature* of a *vegetable; except the inimitable and well observed *sexual system*, and the observations of many authors *on seeds*, which have been *really dissected*. As to the nomenclature and general arrangement of plants, and all that part of botany, I touch not on them.—I respect them as I ought, and only admire. All this will, I fear, appear bold language, especially in a woman: but it is necessary sometimes to be bold in the cause of truth; and I am assured I am defending it. That no person had ever torn down a plant for examination, or followed a plant in the interior from day to day, and from year to year, I am *perfectly persuaded*; and these progressive steps are the *only means* of coming at the *secrets of nature*, and viewing the rising of its seeds from the first moment of their protrusion in their mother's womb, and the formation of flowers from their beginning. But let me not be misunderstood:—I do not mean to say that *innumerable mistakes* will not be found in this general plan of the foundation of plants. As I enter into the details, I have no doubt there will. But I have watched with such carefulness, that I hope they will *be few*. Hitherto I have been most fortunate in not discovering *one gross error*; since I am so particular not to declare a truth till I have investigated it in so many ways, and placed it in so many different lights, that I hope the mistakes will not be *serious at least*. The present truth would not (but for so kind and perfect a voucher) have made its way to the public notice till next year.

It is most curious, that in examining sea weeds I have also discovered the flowers and leaves either filling up the midrib or concealed between the cuticles: but they do not run from the root upwards, they are divided into little *separate* plants with a sort of *root to each*. See fig. 9. the *fucus bulbosus*, where they are to be taken from the midrib by numbers, as when that is torn *up* a number fall out, and will grow on the glass as long as you please to supply them with drops of salt water administered with the fingers. They grow also in the interior of the dulse, or *fucus pinnatifidus*; and if the coralline is first taken off by scraping it with a knife on each side, the flowers will be plainly seen; for, thin as the dulse is, it is covered by a double coralline and many layers of net, besides having the young plants in the interior. These evidently take their nutriment from the lumps of jelly which forms their apparent root, within which grows also a small piece of sea weed—the whole appearing like *Mocha stone* or Egyptian pebble. (See fig. 8.)

At

At the beginning of this letter, I said that the curious formation just discovered, agreed with the general *laws of nature* in the *mineral*, as well as *animal* world. But I should rather, perhaps, have said "they *appear to agree**."

I shall now explain what I mean by the assertion.—All metals when first shooting carry the appearance of *leaves* and *flowers*. But when examined they are always to be divided into the regular forms belonging to each separate *metal*, as *octagonals*, pentagonals, &c. &c. though they carry the appearance of *leaves* and *flowers*: as is seen in the arborescent *silver* ore *from the mines of Potosi*;—in the common iron ore, when combined with salt, at Dawlish;—in the arborescent copper, and zinc tree. But they are all (as I said) to be reduced into their *primitive forms*, and it is merely by their diminutive size, and their attraction for each other, that they carry the appearance of a branch.

But in *animal* life by what principle are they made to receive the same form? I have seen the scale of a beetle when broken, *recover* and *renew itself* by the same means, forming branches thicker and thicker, till the whole became one mass; yet never growing beyond the boundary line given it, the creature being confined for a few days, and regularly examined in the microscope, till the whole scale was completed and restored to its perfect state. I found also a shrimp with part of the claw broken, and evidently renewing in the same manner: but it appeared to me to resemble the metallic manner: that is, to be divided into octaëdrons rather than the coral form of lime: but part of it had grown so thick it was difficult to develop the exact shape†. A fly also, while I detained it in confinement had a part of its wing restored in the same apparent way, and one part of it (some moisture oozing from the interior) absolutely grew after the creature was dead. But I had some most curious trials respecting a butterfly that are most wonderful, but too long for this place. But if any naturalist will try these experiments, he will, I believe, find them just and correct.

* Some aqua-fortis I had by me, became, in *dissolving copper*, *not* of a *blue colour*, but a *fine green*; but adding the mercury, it soon changed to a *beautiful blue*, and made a most admirable mixture; for it might be seen to vegetate in a few *minutes time*, and produced in a few hours vigorous branches of two inches in length: when the branches are formed, it looks exactly like Mocha stone. The experiment is certainly important, as explaining how ramifications are produced in the fissures of slate, flints, agates, Florentine marbles; and, perhaps, even in their very substance at the time of their formation, by the intermixture of saline and metalline particles; and also how metals dissolved by and incorporated with the saline juices of the earth are formed into branches and seem to vegetate.

† Of course these, though belonging to animals, must be classed as of the earths, and divisible into the forms that sort of lime adopts.

Still

Still the vegetable draws its source from a totally different power, from the *vitality* of *that line* on which its life and *existence depend.* When the wing of the fly grew, it was visibly from the juice, and began where the juice fell, and joined itself to the wing afterwards. But the vegetable requires *two things,* the double line a⬤e liquid, without which it cannot increase: for I have watch⬤ with care where the juice was when no line came to it, and it evaporated without any increase ; it is there-fore merely the effect of the moisture on the continued line of life, that creates and excites it to further growth, and the con-tinuation of vegetable life.

I am now going to undertake the *leaf* and *flower-bud.*—Va-rious dissections will be placed before the public, which delinea-tions will, I hope, clear up the whole of this subject, and make it impossible for the most unbelieving really to doubt the facts I have shown. I shall pursue the inquiry in a regular series, by taking the buds from the first of their appearing till they burst out into leaf and flower;—ascertaining every three or four days the contents, and taking drawings of them every week. For this purpose, I shall fix on the ash, the lime, and the oak. I shall also begin this week with the sea-kale and cabbage, to as-certain the *interior flower* of the herbaceous plants,—and the result will all be laid before the public.

I am, sir,

Your obliged servant,

Dawlish, Sept. 29, 1816. AGNES IBBETSON.

Description of the Plate, No. 3. [Plate IV.]

Fig. 1. A specimen of the wood of the *cupressus* cut *longi-tudinally.*

Fig. 2. A specimen of the matter which grew on the glass.

Fig. 3. That which increased from fig. 2 in three days.

Fig. 4. The specimen which was drawn and measured for Sir William Herschel's inspection.

Fig. 5. The same specimen of the wood of the Scotch fir, which increased on the glass from fig. 4 to the present size.

Fig. 6. A specimen of the wood of the *cupressus* cut hori-zontally.

Fig, 7. A specimen of an herbaceous plant cut longitudinally.

Fig. 9. A specimen of the *fucus bulbosus* growing on the glass.

Fig. 8. A specimen of the *fucus pinnatifidus.*

The herbaceous plant has its flower running up from the root inclosed within a circular vessel; which is not the case in trees

or

or shrubs (see fig. 6, *a a a*), but it is very difficult to make them appear within.

††† Mrs. Ibbetson is much shocked at a double mistake made by her respecting the scale of the thermometer, which was Celsus's. (p. 184, Sept.) of which not being accustomed to the scale, and in having her books with her, she was deceived by the person to whom she d. It however only makes the assertion respecting the heat the more impossible to be believed.

⁎ In Mrs. Ibbetson's last communication, p. 181, line 25, *for* " higher regions" *read* " lower regions;" and in p. 185, *for* " approaches" *read* " apparatus."

LVII. *On Sir* HUMPHRY DAVY's *Safe-lamp, and on Flame. By* J. MURRAY, *M.D.*

To Mr. Tilloch.

SIR, — IT has always been my most anxious wish to combat whatever doubts I may have found to exist on the perfect security held out to the miner by the continued use of Sir Humphry Davy's safe-lamp. I have cheerfully descended into the abysses of the mine to exhibit my unshaken confidence in, and entire conviction of, its safety.

That on its introduction prejudice did exist, cannot be denied; but this, the offspring of ignorance, was soon dispelled by the beam of truth. It is difficult to believe that any one nurtured in the paths of science can for a moment honestly doubt of the sufficiency of the security. Let them with me enter the mine, that we may rejoice together. ·

The extraordinary phænomena presented by this discovery were on their announcement rashly questioned, and I am glad of it.—" *Vires acquirit eundo.*" The most sublime discoveries that have adorned the annals of science have had their ordeal to pass, and they have risen with livelier loveliness from the crucible of trial.

In my experiments in the mine I have ventured to place the lamp in the *jet of a blower:*—this would seem to contradict the experiments of Mr. Holmes. In the Dee Bank Colliery, when the fire-damp was flaming in the cylinder, I projected against it a quantity of that highly inflammable powder called *lycopodium;* but even this had no other effect than to increase the *intensity of light.* Like an enraged and imprisoned tiger, it simply gratified the admiring eye with the form it presented and beauty of its appearance. With this shield I could encounter the fire-damp in its most terrific form: and when my services in the cause of humanity are demanded, I think I shall

not

not be found to shrink from the requirement;—and it would rest lightly on me, for the duty will ever please.

Mr. Holmes says that the sentiment excited in my mind by the sublime discovery, should have been applied to Dr. Reid Clanny, and not to Sir H. Davy. My former opinion, however, remains unchanged. Much as I approve Dr. C's exertions in the good cause, still it will not be said that his invention is at all comparable with the instrument projected by Sir H. Davy, for the operations of mining. I would not willingly offend Dr. C. by hazarding any thing which would detract from its merits— *" valeat quantum valere potest ;"*—and yet I must, in deference to Mr. Holmes's opinion, still say, I feel convinced that Dr. Clanny's lamp and the beautiful and perfect instrument of Sir H. Davy ought not to be associated together, nor ever mentioned at the same time. I cannot otherwise appreciate the former.— Independent and essentially distinct, it is unfair to support its pretension by an endeavour to weaken the suffrages so honestly won by its distinguished author Sir Humphry Davy,—a discovery beautifully illustrative of his ingenuity and unwearied perseverance and assiduity. I heartily accord with Mr. Children, that it is not *novelty*, a boyish plaything, which has won our admiration. That it will be permanent too, I with this excellent chemist firmly believe; and may hazard, without the prophetic eye, to pronounce, that it will live to the honour of its discoverer, when we who now exist shall sleep with our fathers; and, to use Sir H's own language, " it will be illustrated by discussion, and be exalted by time." It is indeed not novelty that attracts our regard : we are arrested by a discovery as extraordinary as it is important. It points to humanity for its object, and our hopes beat high ; and in the tone of our exultation we will remember the discoverer. It attaches the signet of experience, and we are satisfied.—What would we have more than this?

We may now fearlessly approach the prison which confines the flame of the fire-damp, wondering at its greatness, and confessing our astonishment at the simple means by which its mighty energies are subdued and made obedient to our wants and wishes; a simplicity which recommends and endears it the more.—And, oh! if possible, let no hallowed prejudices prevent its *immediate* and *universal* adoption! Should not legislative enactment here step in, and raise the voice of authority?

I feel myself highly honoured by the handsome manner in which Mr. Children has been good enough to mention my name in his admirable paper, written as a commentary on Mr. Longmuir's remarks.

The idea suggested, as explanatory of the phænomena of the wire-gauze, by a writer in the last number of the Annals of Phi-

losophy, is made void by my experiments with parallel rods not intersecting to form meshes. I am happy to afford you a correct solution, the inference of actual experiment. In the results of the experiments detailed in a former number of the Philosophical Magazine, I have stated my belief that the phænomena were neither electrical nor magnetic. I that if the rod introduced into the flame of a candle approximate the wick, it is invested with a *cloud of aqueous vapour*, which *fills up the chasm* to which I have alluded. This emanation of aqueous vapour will suffer condensation by contact with the meshes of wire-gauze cooled by the ambient atmosphere ; flame cannot therefore run this gauntlet—for, in fact, the meshes are filled up by a transparent screen of aqueous vapour. The cause of the interval between the surface of metal to which I have in my former paper referred, is now completely and satisfactorily solved.

I have made some experiments with flame—the aqueous particles seem chiefly disengaged from the sides of the cone of flame. The upper part of the spire evolves the charcoal and heated gases. Sir H. Davy observes, that if burning phosphorus is introduced into a large extent of flaming alcohol, its flame will be seen within that of the other : but I observe, if the base of the flaming cone is from one to two inches diameter, that both sulphur and phosphorus, when plunged into it, previously ignited, are severally instantly extinguished; nor does potassium even burn in the interior. I have supposed that probably, when a larger surface is employed, the uneven undulating surface might occasion chasms, through which the exterior air might enter and support the flame : indeed, the following experiment is analogically in favour of the opinion. I adopted *a valve* opening upwards into the cone, and when the alcohol was burning, little puffs of air broke through the lateral surface of the cone.

<div align="center">I am, with high respect, sir,
Your very humble servant,</div>

Stranraer, N. B., Oct. 10, 1816.　　　　　　　J. MURRAY.

. P. S.—Respecting the ignition of gunpowder by flame, I simply alluded to the introduction of it on a slip of ivory into the cone. The cone of flame is not a solid or uniform mass, but is hollow within:—this is proved by pressing the apex by a plate of glass, and looking down through it ; the mantle of flame surrounding the wick will thus appear not much thicker than a wafer.　　　　　　　　　　　　　　　　　　J. M.

LVIII. *Abstract of a Memoir by* M. Leopold de Buch *on the Limits of the perpetual Snows in the North**.

THE determination of the absolute height of the perpetual snows in different latitudes is one of the most important branches of physical geography: this height depends essentially on the climate or mean temperature of each place; so that in order to attain the laws of the distribution of heat at the surface of the globe, it will be sufficient to know the height to which we must ascend under different parallels in order to attain the limit of the snows.

The observations made under the tropics by Bouguer and De Humboldt, those of Saussure, the determinations not less precise of M. Ramond in the Pyrenees,—have proved that near the equator, as in the temperate zone, the lowermost limit of the snows agrees sufficiently with the mean temperature. This is not the case in the north of Europe, where, according to the most recent measurements, the limit of the perpetual snows is more elevated than one could have supposed from the mean state of the thermometer : besides, it is in Norway only that we can immediately observe it. Although the mountains of Sweden are numerous and very high, they attain almost no where the height of the permanent snows; so that this phænomenon is in that country as much unknown as in the greater part of France or Germany.

The observations of M. de Buch were made upon several peaks of that vast chain of mountains which divides Norway throughout its whole length, and extends without interruption from the 58th to the 71st degree of north latitude†. The peak of Saletinds, the elevation of which above the level of the sea is 1794 metres, exceeds very little the limit of the snows : M. de Buch fixes it at

* Annales de Chimie et de Physique for June 1816, tome ii. p. 188.— This interesting paper was read by M. de Buch to the Institute in March 1810; but it was not printed until 18·6: at the conclusion of a French translation (by M. Eyriés) of M. de Buch's Travels in Norway and Lapland. We regret that the limits of this Journal compel us to abridge the present memoir : we have taken care, however, to suppress nothing important, and have adhered as closely as possible to the words of the author.—*Note by Messrs. Gay Lussac, and Arago, editors of the Annales de Chimie et de Physique.*

† This chain yields to very few mountains in Europe in point of height, and surpasses them all by its extent and mass. When we traverse the Alps or the Pyrenees, and scarcely arrived at the Passes we commence suddenly to descend, we know of no pass exceeding a league in breadth. On the Long Field in Norway, on the contrary, when we have ascended a valley to its origin, we see a platform or ridge stretching out, the height of which is almost every where 1400 metres above the level of the sea, and the breadth eight, ten, and even twelve leagues. It is impossible to traverse the chain in one day : the inhabitants of the west side, who must pass over these deserts to go into the eastern provinces, are obliged to pass the night there, at the risk of losing themselves in everlasting mists, and perishing of cold in the midst of tempests and whirlwinds of snow.—*M. de Buch.*

1690 metres, the latitude being about 61°. This height is less
(only 1597 metres) in the chain nearest the sea, which is called
Folge-Fonden-Field. Finally, the Melderskin, still nearer the
ocean, constantly retains the snow, and yet its peak is 209 me-
tres *below* the limit at which we observe the same phænomenon
on the Great Chain.

In order to account for this gradual lowering of the limit of
snow in proportion as we approach the ocean, M. de Buch re-
marks that the prevailing winds on the coasts of Norway are al-
ways south-west and south; northerly and easterly winds are there
infinitely less frequent, and feebler. Now the former come from
the warm regions : in passing over the sea they are saturated with
humidity; but being soon cooled by the continents, a part of
the dissolved water is precipitated in the form of mists, clouds,
and, finally, of those torrents of rain which inundate the islands si-
tuated along the shore*: the sun penetrates but rarely this almost
perpetual bed of clouds, and its rays warm the ground but slightly;
the temperature of the warmest months ought consequently to be
less near the shores than in the interior, where the sun during
the long days of summer exercises a great influence. There is
therefore every year less snow melted on the mountains which are
near the sea; and the limit at which it keeps unmelted ought to
be lowered much.

Another cause, of which Saussure had already perceived the in-
fluence, is the mass of mountains. If the snow occupies a great
extent, it lowers considerably the temperature around, and thereby
prevents the lower snows from melting at elevations at which it
would melt on isolated peaks. M. de Buch finds in the form and
situation of the chain of Fonden Field, something on which to
found a fortunate application of those considerations to the re-
markable phænomenon which he wished to explain.

If we quit the countries just mentioned, and proceed 10 de-
grees further northward, *i. e.* to the extremities of the European
continent, we expect to meet with the snow limit almost at the
surface of the ground ; but the general aspect of the country soon

* There never falls at Bergen in the space of a year, less than 68 inches
of rain, and frequently 92 have been known to fall: whereas at Upsal, in the
same latitude, but in the interior, the annual quantity of rain does not exceed
14 inches. The currents of heated and humid air which produce these
great differences also exercise a remarkable influence in the gravity of the
atmosphere. M. Hertzberg of Kynservig, by using excellent syphon baro-
meters, never saw, for ten years, the mean annual height of the barometer
exceed 28 inches and half a line (0·7592m). This result is confirmed by the
observations of M. Stroem, under the 63d degree, and by those of M.
Schytte in the 68th : and it will appear still more curious to learn that at Pe-
tersburg, Abo, and Stockholm, in the Baltic, the mean annual height of the
barometer frequently rises to 28 inch. 3 lin. = 0·7646m.—*M. de Buch.*

shows

shows that this is not the case. We find, in fact, under the 70th degree of latitude, fields and gardens well cultivated; a numerous population also covers the shores of the great arms of the sea, and beautiful forests grow in the valleys. It is towards the extremities of Lapland that the immense chain of mountains of Norway divides and disappears. One of the latter peaks, the Akka-Sokki, situated in the interior of the Gulf of Alten, was not covered with snow when M. de Buch ascended it on the 16th of August 1807, and nevertheless, according to the barometrical observations, its height above the sea is 1023 metres : but an adjoining mountain, the Storvauds Field, retains the snow the whole year; its height is 1071 metres :—hence it results that in the 70th degree the limit sought ought to be nearly 1060 metres. This height is, as we see, considerable; it equals that of the Puy du Dome, above the ridge of Clermont, and surpasses the height of the principal mountains of Germany. Henceforth we ought no longer to be astonished that at the level of the sea, *i. e.* 1000 metres below the snow limit, vegetation has still some vigour and the forests extend to great heights: besides, the heights at which the various species of trees and shrubs cease to grow are very clearly defined. The limits of the pines and birch-trees never vary beyond 30 metres, and exhibit themselves like lines of demarcation cut along the sides of the mountains. We dwell upon this observation (in all respects so curious) the more forcibly, as M. de Buch made use of it to determine the snow limit at the North Cape.

The following is the table of his results :

	Metres.
The Pine (*Pinus sylvestris*) disappears at	237
The Birch-tree (*Betula alba*) - -	482
The Myrtle (*Vaccinium Myrtillus*) -	620
The Mountain Willow (*Salix myrsinites*)	656
The Dwarf Birch (*Betula nana*) - -	836
The snow ceases to melt at - -	1060

There is therefore a difference of 245 metres between the limit of the pine and that of the birch-tree, and 578 metres between the limit of the birch-tree and the snow limit. These relative differences are the same in Norway and Lapland, although the absolute heights of the limits are different. Thus we see the pines disappear at 980 metres; we must rise to 1225 metres = 980 +245 in order to find the limit of the ——, and the snow limit will be at 1803 metres = 1225 + 578.

We might easily, by setting out from these data, determine the height of the snows on the remotest islands towards the frozen sea; although, considering the little elevation of the mountain, the snow does not remain the whole year. Thus near Hammer Fest, the last city of Europe to the northward, we find birch-trees under the form of weak shrubs at a height of 227 metres;—at Ma-

geroë (the island where Cape North is) we see no traces above
130 metres: the snow limit will therefore be 805 metres = 227
+578 at Hammer Fest, and 708 metres = 578+130 at Cape
North. The rocks of this last point are only 390 metres high, so
that it would require 318 metres more in order that the snow
should not melt in summer.

One remark of great truth we owe to M. de Buch, namely,
that the snow limit ought to depend chiefly on the temperature
of the months during which the snows usually melt; so that it is
not the mean temperature which determines its height. Thus in
the interior of the Gulf of Alten, the mean degree of the thermo-
meter is less than at Cape North*, and yet the snow limit there
is much higher. This is because one and the same mean *annual*
temperature may result from very different *monthly* mean tem-
peratures; as we may be convinced by the subjoined table, which
presents, month by month, the mean temperatures of Mageroë
(an island of Cape North), and of Uleoborg, under the parallel of
Torneo.

Mageroë,	lat. $71\frac{1}{2}°$	Uleoborg, lat. $65°$
January,	— 5·5°	— 13·5°
February,	— 4·9	— 10·0°
March,	— 4·0	— 9·9
April,	— 1·0	— 3·2
May,	+ 1·1	+ 4·9
June,	+ 4·5	+ 12·9
July,	+ 8·1	+ 16·4
August,	+ 6·0	+ 13·7
September,	+ 3·1	+ 8·0
October,	0·0	+ 3·7
November,	— 3·5	— 5·2
December,	— 3·5	— 10·2
Mean result =	+ 0·03	= + 0·63

We see that the definitive mean temperatures of the year differ
little from each other in these two places, and yet the mean of the
positive temperature rises at Uleoborg to 10°, whereas it does
not go beyond 4° at North Cape. Those considerations add to
the interest which ought to be excited for the determination of
the snow limit:—if its height in fact is only regulated, as we have
said before, by the summer temperature, it will become in some

* The mercury often freezes in the open air at Alten, but never at Cape
North. The thermometer falls every winter at Alten to 25° below zero. At
Cape North it is only —12°, or at most —15°, and —17·05 is the extreme
point. Thus the sea never freezes in those regions. We must be 20 or 30
leagues from the latter promontory before we see the islands of ice, and
still they are very far off in the horizon.—*M. de Buch.*

respects

respects a measure for the strength of vegetation, which must depend only on temperatures above zero. .

The height at which the perpetual snows are found under different latitudes, and the distance from their limit to that of the trees, have been lately the subject of a profound discussion in the *Prolegomena distributione Geographica Plantarum* of M. de Humboldt. We shall give in a future number of the Annales de Chimie et de Physique an extract of that part of the work which contains some new and ingenious considerations on the direction of *isothermal lines.* At present we shall confine ourselves to such of M. de Humboldt's results as refer to the interesting question which is the subject of the article.

1. Between the tropics from 0° to 10° latitude in the cordilleras of the new world, the limit of the snow = 4795 metres (2460 fathoms). The mean temperature of the air at this height is not zero, as Bouguer and after him all have admitted, but rather + 1·5° centigrade.

2. Between the parallels of 19° and 21° of north latitude, Mexico, at the entrance of the torrid zone, we find perpetual snow at 4580 metres (2350 fathoms).

3. Under the temperate zone, on Caucasus lat. 42°—43°, the height according to Messrs. Englehardt and Parrot is 3216 metres (1650 fathoms).

4. In the Pyrenees (lat. 42°½—43°), M. Ramond finds the permanent snows 2729 metres (1400 fathoms*). At this height the mean temperature of the year = — 3·5°.

5. The

* We might have been astonished to find a difference of 487 metres (250 fathoms) between the height of the snow on Caucasus and the Pyrenees, almost under the same parallel, if the discussion to which M. de Buch devotes his memoir had not shown how great is the influence of local circumstances. The following passage, taken from a letter, now very old, of M. Ramond, will have the double advantage of affording a considerable degree of certainty as to the determination of altitude which the text contains for the Pyrenees, and of making known the peculiar causes which in this chain render the observation difficult.

" I have remarked that the inferior snow limit descends lower the higher the mountains rise, and that the mountains which form the skirts of a chain are deprived of snow at a height at which the centre mountains are covered.

" Thus the Peak *du Midi*, although 2935 metres (1506 fathoms) high, has a permanent snow, if it is not in a ravine exposed to the north-west, and still it disappears in warm summers. Neouville and Peak Long have permanent snows at 2826 metres (1450 fathoms) only, and these snows contain glacieres which never appear above them. But in order to have the characteristic limit of the Pyrenees we must go to the centre of the chain, *i. e.* to the environs of *Mont Perdu, Marboré,* and *Vigue Male.* I remark as follows in places favourable to those kinds of observations :—there are carpets of permanent snow above the *Port de Pinede, Col de Nixelle,* the lake of *Mont Perdu,* and above the peaks of *Port de Garvarnia,* &c. The mean estimate

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5. The mean of the observations recently published by M. Wahlenberg gives for the snow region in the Alps (lat. $45°\frac{3}{4}$ —$46°\frac{1}{4}$) 2670 metres (1370 fathoms). At this height the mean annual temperature $= -4°$, the mean of winter is $-10°$, that of summer $+ 6°$.

6. The mean temperature of the year, at the height at which M. de Buch has found the limit of the snows under the 68th degree of latitude, is $-6°$; that of winter $= -20·5°$, that of summer $+9·5°$.

From the parallel of Popocatepec in Mexico, to that of Etna, the inferior limit of the snow has not yet been determined by direct measurements. According to the researches of M. de Humboldt, this limit, by the latitude of $28·17°$, which is that of the Peak of Teneriffe, must be 3800 metres (1950 fathoms), but the height of this ridge is only 3711 (1904 fathoms); so that if it is clear of snow during summer, it is less on account of being heated (as was thought) by the volcanic fire, than on account of its elevation, which is not considerable enough.

In order to show how much this department of general physics has gained latterly by the researches of travellers, we shall subjoin the heights which Kirwan and all subsequent authors assign to the limits of snow, and by their side the results which have been furnished by direct observations :

Latitude.		Height of the Snow Limit from actual observation.		Height of the Snow Limit according to Kirwan.
		metres.		metres.
0°	..	4795	..	4783
20°	..	4580	..	4186
45°	..	2729	..	2343
61°	..	1690	..	....
62°	..	1582	..	943
65°	..	....	..	766
67°	..	1169	..	....
67½°	..	1072	..	619
70°	..	1060	..	....

of these elevations is about 2632 metres, (or 1350 fathoms), and all things considered, I do not think it ought to be fixed lower: perhaps it may even be carried the length of 2729 metres (1400 fathoms), for below this the carpets of snow are not continued, and their being maintained depends on local circumstances, of ravines, or exposure, &c. Besides, nothing is more difficult than to establish this fact in the Pyrenees, because the whole centre of the chain is composed of mountains vertically broken : there are almost no moderate slopes ; the snows roll down and form heaps which resist the warmth of summer by their thickness, whilst the heights, which ought to be covered with them, are bare."

LIX. *On Cast Iron and Steel; with Experiments to ascertain whether Manganese may be alloyed with Iron.* By DAVID MUSHET, *Esq. of Coleford, Forest of Dean.*

To Mr. Tilloch.

SIR, — As soon as the following analysis of Swedish iron and steel, by Bergman, was generally known to the iron-trade of this country, an expectation prevailed that by combining manganese with the ironstones or iron ores used at our own furnaces, or by the discovery of an iron ore already combined with manganese, a quality of cast iron would be produced, that might be manufactured subsequently into bar iron fit for making good steel.

Cast iron	Plumbago	2·20	
	Manganese	15·25	100 parts.
	Siliceous earth	2·25	
	Iron	80·30	

Steel	Plumbago	·50	
	Manganese	15·25	100 parts.
	Siliceous earth	·60	
	Iron	83·65	

Bar iron	Plumbago	·12	
	Manganese	15 ·25	100 parts.
	Siliceous earth	·175	
	Iron	84·788	

I am not aware of any regular series of experiments having been performed on the large scale, to justify the general expectation. The late Mr. Reynolds used manganese, not in the smelting furnace, but in the subsequent operation of refining the cast iron, doubtless with a view to effect a combination of the metal of manganese with the iron, corresponding to the above analysis. The steel manufactured from this iron was in point of quality, I believe, superior to any formerly made from coke iron in this country, but I believe that no direct experiment was made to ascertain whether any metallic manganese, and what quantity, became united to the iron.

Steel-iron and steel have since been manufactured to some extent near Ulverston, not with coke but wood charcoal, and the quantity or fitness of the iron for steel-making attributed directly to a portion of manganese in the ore.

In the above analysis there is a decided anomaly, which of itself would be sufficient to lead to a suspicion of the accuracy of the result. If in the first place it is admitted that the original crude iron contained 15¼ per cent. of manganese, it is extremely improbable, from the superior affinity of manganese for oxygen, that no extra proportion of this beyond that of the iron should

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be wasted or burnt out during its exposure to the blast of the
refinery; and that the resulting bar iron should still contain the
same quantity of 15¼ per cent. originally in the cast iron.

The French chemists very early doubted the presence of man-
ganese in iron and steel, and subjected some of their best steel to a
rigid analysis.   I find the following record of their experiments by
Vauquelin, in Nicholson's Journal, quarto series, vol. ii. p. 212.

Steel, No. 864	Carbon	.. ..	0·00789
	Silex	.. ..	0·00315
	Phosphorus	..	0·00345
	Iron	.. ..	0·98551
			1·00000
No. 864 (Large piece)	Carbon	.. ..	0·00683
	Silex	.. ..	0·00273
	Phosphorus	..	0·00827
	Iron	.. ..	0·98217
			1·00000
No. 977.	Carbon	.. ..	0·00789
	Silex	.. ..	0·00315
	Phosphorus	..	0·00791
	Iron	.. ..	0·98217
			1·00000
No. 1024.	Carbon	.. ..	0·00631
	Silex	.. ..	0·00252
	Phosphorus	..	0·00520
	Iron	.. ..	0·07597
			1·00000

The approximation of the results of these experiments to each
other, in which the presence of manganese was not at all disco-
vered, is a strong proof of their accuracy: but as Vauquelin and
Bergman in no case seem to have analysed the same iron, it
does not prove the non-existence of manganese in the Swedish
iron analysed by the latter.   Those who consider the excellence
of steel to be derived from the presence of metallic manganese,
may deduce an inference from the experiments of Vauquelin in
favour of their own theory; as it is confessed in the comparative
experiments detailed in that memoir, that the steel of France
will not stand comparison with the best steel of England: the
steel of this country being always manufactured from the best
                                                        Swedish

Swedish irons. As far as my own experiments go on the subject of analysing by deoxidation and fusion the ores from which the Swedish steel are manufactured, I have not found any indications of a larger proportion of manganese than I found in the ores of this country. Nor have I at any time been able to satisfy myself that presence of manganese imparted any peculiar excellence to the steel produced in these experiments.

At the time when the alloys of manganese with iron excited considerable interest in this country, I made some experiments on the subject, which if you consider at this late period fit for insertion in your Magazine, they are much at your service.

I first attempted to prove whether it was probable that metallic manganese united to iron in the process followed by Mr. Reynolds.—For this purpose I selected a quantity of cast iron boring dust, with which the following experiments were made:

Grains.

1. Boring dust 400
       This was fused alone in a clay crucible, and the
    result was a mottled button of cast iron, weighing 382

                                        ——————
                   Loss equal to 4½ per cent. .. 18.
       This experiment repeated (by simple fusion) made the
    loss 20 grains, or 5 per cent.
2. Boring dust 400
   Manganese ⅛th (black oxide) 50
       A small portion of brown glass was formed in the fusion.
    The metallic button weighed 382 grains; loss, 18 grains,
    or 4½ per cent. of iron.
3. Boring dust 400
   Manganese (black oxide) ¼th 100
       The result of the fusion of this mixture was a button of
    cast iron of a white fracture and minute steely grain;
    weighed 375 grains.
       Loss 25 grains, or 6¼ per cent.
       The glass was yellowish, opaque, and dense.
4. Boring dust 400
   Manganese (black oxide) ½ .. .. .. 200·
       Result of this fusion metallic button weighing 367 grs.
       Loss 33 grains, or 8¼ per cent.
       Fracture, that of dense hard steel, grain blueish dull, similar to steel made by decarbonation from cast iron. Glass
    deep brown, opaque, though in thin fragments possessed of
    some transparency.
       The general result of these experiments in close crucibles by no means indicates a combination of metallic
                                               manganese

manganese with crude iron; but they prove a decarbonation from mottled iron to steel, and a loss of matter, by oxidation and decarbonation, of 4½, 5, 4⅞ 6¼, 8¼ per cent.

Grains.

5. Boring dust   400
    Manganese (black oxide, which had ⅛st 22 per
    cent. of oxygen in a red heat) ⅛th   50

     The metallic button obtained from this fusion was in fracture perfect white cast iron, but possessed of a blueish glance similar to No. 3; weight 382 grains; loss 18 grains, or 4½ per cent. Glass deep brown with occasional blue streaks.

6. Boring dust   400
    Manganese deoxidated, ¼th   100

     The metallic button obtained, weighed 386 grains; loss 14 grains, or 3¼ per cent.; fracture, steel-grained, glass deep brown, opaque.

7. Boring dust   400
    Manganese deoxidated, ½   200

     Result, a smooth metallic button covered with a minute chequered crystallization; weight 386 grains; loss 14, or 3¼ per cent. The fracture of this button was uncommonly dense, and impressed with the same crystallization that was noticed upon its surface. By comparing this result with No. 4, where the same quantity of oxide of manganese was used, it appears extremely probable that a portion of metallic manganese was here united to the iron nearly equal to 4¼ per cent. the difference of the metallic waste sustained in the two fusions.

8. Boring dust   400
    Manganese deoxidated   400

     A metallic button similar to No. 6, was obtained from this fusion; weight 369 grains; loss 31, or 7¼ pet cent.; fracture, grayish blue, steel-grained. If compared with No. 4, where only half the quantity of manganese was used, the metallic oxidation is less ½ a per cent, rendering it still more probable that a portion of metallic manganese in this, as in No. 9, became united to the iron in fusion. But this quantity seems so small for experiments performed in close crucibles, that I am inclined to think, in the refining of cast iron, as practised by Mr. Reynolds, in an open furnace with oxide of manganese, that no metallic alloy took place with the iron.

     With a view to investigate the subject further, and to prove what effect in the furnace would be produced by mixing

ing charcoal with the manganese, I attempted to fuse
400 grains of the same boring dust with 40 grains of char-
coal. The result was imperfect, owing to the iron and char-
coal forming a carburet, which was infusible in the highest
heat of the furnace. With 15 grains charcoal and the same
quantity of boring dust, a fine button of carbonated smooth-
faced iron was obtained, weighing 391 grains; loss 9 grains,
or 2¼ per cent.; fracture blackish gray.

9. Boring dust .. .. •• .. .. 400 grs.
   Manganese oxide 100
   Charcoal 40

   This fusion afforded a dense button of mottled cast iron,
weighing 393 grains; loss 7 grains, or 1¾ per cent.; there
remained unreduced 10 grains of magnetic black carburet
of iron. In this experiment 2 grains extra of metal are
obtained from the manganese, and a decarbonation expe-
rienced from very gray to mottled cast iron.

10. Boring dust 400
    Manganese oxide 200
    Charcoal .. .. •• 40

    This fusion afforded a dense button of white cast iron,
the fracture of which indicated a greater portion of metallic
manganese in alloy than had hitherto been obtained in
these experiments; weight 401 grains. In the fusion of
400 grains of boring dust with 15 grains of charcoal, 391
grains of iron was obtained: in the present experiment the
result was 401 grains; which is an increase of 10 grains of
metal derived from 200 grains of manganese in consequence
of mixing with it 40 grains of charcoal. This present me-
tallic button may therefore be considered as being com-
posed   Of iron   ..   ..   ..   97·5
           Manganese   ..   ..   ..   2·5
                                            ————
                                            100 pts.

    In this experiment the metallic button was found covered
with a party-coloured glass, milky blue, brown, and gar-
net; 3 grains of magnetic carburet of iron found unreduced.

11. Boring dust .. .. 400 grs.
    Manganese oxide, ... 400
    Charcoal 40

    The result of this fusion was a fine crystallized metallic
button weighing 390 grains; loss 10 grains, or 2¼ per cent.
the effect of the charcoal as to metallizing the manganese
seems to have attained its maximum in No. 10. The glass
now obtained was brownish red, without any carburet on
its surface, as in Nos. 9 and 10.

These

These experiments make it evident, that metallic manganese may be united to cast iron by mixing it with a portion of charcoal; and it is probable that a similar effect would take place in the puddling furnace. But as in these experiments with the charcoal in mixture, the iron was not decarbonated or refined enough, it cannot be positively inferred that the metallic manganese would remain alloyed with the iron during the whole process of refinement.

I am, &c.

DAVID MUSHET.

LX. *On the Cosmogony of Moses.* By J. C. PRICHARD, *Esq.*

To Mr. Tilloch.

SIR, — YOUR correspondent F. E——s has vainly flattered himself with the idea of having extorted from me a confession which I have not made. Far from allowing that I never supposed him to have uttered the assertion I had imputed to him, I pointed out the particular passage in which that proposition is to be found, and where it is expressed in words the sense of which can only be eluded by a mere quibble.

F. E——s having at length abandoned his attack on all the points which I have been anxious to defend, and contenting himself with taking up a position on wholly different ground, I am very willing to leave him in undisturbed possession of his new quarters.

I have the honour to be, sir,
Your obedient servant,

Bristol, Oct. 14, 1816.   J. C. PRICHARD.

LXI. *Notices respecting New Books.*

Transactions of the Geological Society, vol. iii. 4to. 452 pp.

WE have great pleasure in announcing the appearance of a volume, rich in matter as well as embellishment (being accompanied with twenty-six plates and maps bound up separately), so that it cannot but prove acceptable to every geologist. The contents are as follow:

I. A Sketch of the Mineralogy of Sky. By John MacCulloch, M.D. F.L.S. President of the Geological Society, Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich. 111 pages.—II. On the Oxyd of
Uranium,

Uranium, the production of Cornwall. By Mr. Wm. Phillips, Member of the Geological Society. 9 pages.—III. On the Geological Features of the North-eastern Counties of Ireland. Extracted from the Notes of J. F. Berger, M.D. Member of the Geological Society; with an Introduction and Remarks, by the Rev. W. Conybeare, Member of the Geological Society. 75 pages. —Descriptive Notes referring to the Outline of Sections presented by a part of the Coasts of Antrim and Derry. By the Rev. W. Conybeare, Member of the Geological Society. 27 pages.— IV. On the Dykes of the North of Ireland. By J. F. Berger, M.D. Member of the Geological Society. 13 pages.—V. Some Remarks upon the Structure of Barbadoes, as connected with Specimens of its Rocks. By Joseph Skey, M.D. Physician to the Forces. 7 pages.—VI. Outlines of the Geology of Cambridgeshire. By the Rev. J. Hailstone, F. R. and L.S. Woodwardian Professor in the University of Cambridge. 8 pages.— VII. Some Observations on a Bed of Trap occurring in the Colliery of Birch Hill, near Walsall, in Staffordshire. By Arthur Aikin, Esq. Secretary to the Geological Society. 7 pages.— VIII. A Geological Description of Glen Tilt. By John MacCulloch, M.D. F.L.S. President of the Geological Society, Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich. 8 pages.—IX. Sketch of the Geology of the South-western Part of Somersetshire. By Leonard Horner, Esq. F.R.S. Member of the Geological Society. 79 pages.—X. Description of a Clinometer. By Lord Webb Seymour, F.R.S. F.R.S.E. F.L.S. Member of the Geological Society. 47 pages.—XI. A Sketch of the Geology of the Lincolnshire Wolds. By Mr. Edward Bogg, Land Surveyor. 7 pages.—XII. On the Tremolite of Cornwall. By the Rev. W. Gregor, Honorary Member of the Geological Society. 5 pages.—XIII. Some Observations on the Salt Mines of Cardona, made during a Tour in Spain in the Summer of 1814. By Thomas Stewart Traill, M.D. Member of the Geological Society. 9 pages.—XIV. Description of a new Ore of Tellurium. By Professor Esmark, of Christiania. Foreign Member of the Geological Society. 2 pages.—XV. An Account of the Swedish Corundum, from Gellivara in Lapland. By C. T. Swedenstierna. 6 pages.—List of Donations, Description of Plates, and Index.

The first paper, by Dr. MacCulloch, displays equal industry and intelligence. Though modestly given as *a sketch* it is replete with information, and exhibits many new facts well deserving of attention, and which, if properly followed up, will tend to correct some opinions which have perhaps been too hastily adopted. It cannot be doubted, that hypotheses absolutely

lutely groundless, have often occasioned investigations which have led to theories founded on truth; but it will not in the present age be denied that a too ready adoption of systems without sufficient facts to support them has been the bane of science, and operated to retain mankind in a state of ignorance. The facts stated in this paper furnish various "objections to the commonly received arrangement" of strata, and "enforce on geologists the necessity of drawing their distinctions from real and not from theoretical [hypothetical] views, and of establishing criteria which are better founded, and which rest on more satisfactory evidence than that produced by the mere apparent or even real superposition of an unstratified above a stratified rock." We regret that our limits do not allow us to offer an enumeration of the new facts established by Dr. MacCulloch's Sketch of the Mineralogy of Sky; but his "detailed description of the marble of Strath" should be seen by others as well as geologists, were it only for the "œconomical uses to which it seems applicable," and we therefore give it in his own words :

"The following varieties are the most remarkable of those which are to be seen in this tract.

"1. Pure white marble, the fracture intermediate between the granular and small platy.

"2. The same with a scarcely discernible shade of gray.

"3. The same with variously disposed veins of gray and black, resembling the common veined marble used in architectural ornaments.

"4. The same with narrower veins well defined, and often reticulated with a great semblance of regularity: very ornamental.

"5. The same, distinguished, independently of the veins, by a parallel and regular alternation of layers of pure white and grayish white.

"6. White marble variously mottled and veined with gray, yellow, purple, and light green. - This is also a very ornamental variety.

"7. Marble, exhibiting various mixtures of white, pink, purple, light green, dark green, and black, of a rich sombre effect, and highly ornamental.

"8. White marble, beautifully mottled and veined by yellow transparent serpentine.

"The ornamental coloured marbles here described, scarcely yield in beauty to many of the similarly constituted specimens of ancient marbles, and like many of the marbles of Scotland they will be found to owe their colours to serpentine. This is also the case in Glen Tilt, at Balahulish, and in Iona. But the

most

most obviously valuable variety is the white, which seems to possess most of the qualities requisite for the purposes of statuary.

" Few substances in the catalogue of those with which œconomical mineralogy is concerned have excited more interest than statuary marble, from its rarity, its beauty, and its indispensable necessity in the art of sculpture. It has at different times formed an object of anxious research in this country, and premiums have been held out for it by the Society of Arts. It has consequently been found in various parts of Scotland, as well as in Ireland, but no native specimens have yet been introduced into the arts. As the causes which have impeded their introduction have hitherto been such as may be considered adventitious, being of a commercial nature, and not founded on any experience of their physical defects, it has been hoped that they might by perseverance and time be removed, and that those statuary marbles of this country might at some future day supersede the necessity of importing this article. It will not therefore be a misplaced inquiry to examine the several properties of those marbles which have at different times held a place in the estimation of artists, and to compare them with our own specimens, more particularly with that of Sky, now under review, the most abundant and certainly the most specious of all those which have yet been found in Britain. The inquiry is the more necessary, as the several circumstances in which white marbles differ, do not appear to have been generally attended to, and as an undue value seems in some instances to have been fixed on our own in popular estimation, although not in that of sculptors themselves.

" The value of this substance in those distant periods when the arts of Greece flourished, occasioned an industrious research after a material in which the sublime ideas of its artists could be embodied. Accordingly many quarries have been wrought in ancient times, of which little has descended to us but the names, and a few of the works which were executed from their produce. These marbles were of various qualities, and examples of them are still to be seen in ancient statues; although with regard to many of them, a species of evidence, often little better than conjectural, has guided sculptors and mineralogists in their attempts to determine the quarries from whence they were derived. Among these, the quarries of Parose afforded a marble (the often-quoted lychnites of Pliny) in which it is asserted that the celebrated Venus was wrought, as well as some others to which we have not access. But there are many specimens of sculpture in the British Museum which seem to have been executed in this stone, or in one at least of analogous character.

" Of

" Of the nature of the Parian marble we are enabled to speak positively, since some blocks of it have been quarried during the last few years, and are now to be found in the shops of the sculptors of this city. The grain of this marble is large and glistening, while at the same time its texture is loose and soft, and its colour of a yellowish and watery white. It possesses considerable translucency on the edges, a quality which, however desirable in statuary marble when of a fine grain, from the softness which it gives to the outline, only increases the disagreeable aspect of the Parian, by the angular reflections of light which take place on the pellucid edge and surface, from the innumerable faces of the small plates. The specimens of sculpture which I am about to quote, will exemplify this fault. It is certain indeed that the Greek sculptors abandoned the marble of Paros after the quarries of Luna and Carrara were discovered, the superior fineness and whiteness of these marbles, which at present cause them to excel any with the places of which we are now acquainted, rendering them also at least equal to the best of those ancient ones of which the native places are now unknown.

" Independently of the injurious effects which the large grain of the Parian marble produces on the transparent surface of sculptured works, and the false lights which it thus introduces into the contour, it interferes materially with the requisite correctness of drawing in the lesser works, and is thus inapplicable to the details of small sculptures in relief. It is nevertheless susceptible of a good polish, a quality, however, of little value in the eyes of the statuary, and one which in this variety only serves to render the defects of its texture more apparent. It is also said to have been deficient in size, since it was so intersected by fissures as to be incapable of yielding blocks of more than five feet in length. I may add, that in the present state of the public habits with regard to white marbles, there is no demand for modern works executed in Parian marble. Its celebrity is consigned to the metaphors of poets.

" It will afford satisfaction to those who are interested in the arts to point out such works in the British Museum as appear to have been executed in Parian marble, or in one of similar character.

" A Cupid bending his bow. This specimen is rather of a finer grain than the generality. It may perhaps belong to that marble called by the Italian sculptors *murmo statuario*, but this question cannot be determined without a fresh fracture.

" A bust of Minerva.

" Aratus, a bust. This also is of a fine grain like the Cupid.

" A Venus,

" A Venus, of a similar grain, and agreeing with the character of the marmo statuario.

" Zeno, a bust, of a very coarse grain.

" A terminal head of Bacchus.

" A terminal head of Mercury.

" A Jupiter Serapis.

" Bacchus and Ampelus.

" Marcus Aurelius, a bust.

" There are others, but it is not requisite to enumerate them.

" A marble of a much finer grain, and capable of a high polish, is described by the antients, as found near the river Coralus in Phrygia, as well as in some of the Greek islands : it is supposed to be the variety known to statuaries by the name of marmo Greco, and some ancient statues are described as being formed of this marble. It is possible that specimens of it may exist in the British Museum, but our sculptors are, as far as I know, incapable of distinguishing it at present, and it is much too hazardous to assign the place of a particular specimen from the contemplation of a polished and often of a stained surface. Mr. Tennant has found that the marmo Greco is a magnesian limestone.

" I am equally unable to point out specimens of that variety known to the Italians by the name of marmo statuario, of which the quarries are also lost, but which, with greater translucency of surface, resembles the Parian marble in the largeness of its grain, unless those which I have conjectured to belong to this variety, when describing the specimens of sculpture in Parian, do in fact appertain to the latter.

" The quarries of Luna produce a compact white marble susceptible of a high polish, and capable of being wrought with the most minute accuracy. Hence it is preferable for the finer operations of bas-relief, either to the Parian, of which the aspect interferes with the delicacy of finish and of surface required in these works, or to the Pentelic, which was subject to accidents from veins of mica and of serpentine, or to that of Carrara, in which dark veins are of frequent occurrence. It was accordingly preferred by the antients, and among many other works, the Apollo (Belvedere) is said to have been executed in Luna marble. We have no other knowledge of the marbles of Hymettus and of Arabia than their names.

" Of all the marbles employed in the works of the antients, and of which many specimens have descended to our days, that of Carrara is almost the only one which is at present held in estimation, or is now accessible to modern sculptors. This marble is of a very fine grain and compact texture ; it is also susceptible of a high polish when required, and is consequently applicable to every species of sculpture, except when, as is too often the case,

dark veins intrude and spoil the beauty of the work. Notwith-
standing the general apparent uniformity of its texture, it offers
different varieties of aspect. It is always of a fine granular fracture,
yet this fracture is sometimes combined with a slight tendency to
the flat splintery, in which case the stone is harder and more trans-
lucent than when it is purely granular. When merely granular it
is sometimes dry and crumbly, precisely as if it had been exposed
to a high heat; it then loses much of its transparency, and is
called *woolly* by sculptors. Its transparency is various, and in
some cases nearly equal to that of alabaster, (granular gypsum).
    " The bust of Marcellus in the Museum offers an example of a
very fine grained and extremely translucent marble, apparently of
this kind. The specimen employed in the bust of Messalina is
equally remarkable for the fineness and beauty of its texture. In
a bust of a youthful Hercules in the same collection the identity
of the marble is marked by the dark veins which are to be seen
in it; but it is unnecessary to quote individual specimens, as the
greatest number of the sculptures in this collection appear to have
been executed in Carrara marble.

    " The last of the antient marbles which I shall describe is that
of Pentelicus, of which the quarries are probably still to be found
in the vicinity of Athens, although they have not been investigated
by modern travellers. But we are in possession of numerous
specimens of sculpture in this stone, from which we are able to
determine its qualities; two are to be seen in the British Museum.
Of these there is the bust of a Philosopher, of, apparently, antient
and very dry workmanship : the other is the celebrated Disco-
bulus. It is known that Myron the Athenian, who flourished
about 440 A.C. executed a work of this character in bronze :
but we have no evidence respecting the marble statue, and artists
have therefore remained in doubt whether it was executed by him-
self or was a copy by another hand. This question cannot be
positively decided by the sculpture itself, however high its merits.
In the mean time a step is gained by the mineralogical investi-
gation of the material, and thus mineralogy is capable of throwing
light on the history of the arts. The substance in which it is
wrought must therefore be considered a sufficient proof of the an-
tiquity of the copy, if it be such, as well as of its having been ex-
ecuted at Athens, since the quarries of Pentelicus were abandoned
in consequence of their defects, as soon as those of Carrara and
Luna were known. Although it is difficult or impossible to de-
termine this period, yet as so few works in Pentelic marble poste-
rior to the time of Phidias and of Myron have descended to us, it
is probable that little use was made of those quarries after the pe-
riod of these artists. We are therefore, perhaps, entitled to con-
clude that the Discobulus of the Townley collection is an Athe-
                                                                    nian

nian work of the best age of soulpture, and not a copy by any more modern artist ; and that if it was executed neither by Myron himself nor under his direction, it is yet not likely to have been much inferior to the original, while it may serve, at the same time, as some proof of the esteem in which that work was held at Athens.

But the most numerous examples of Pentelic marble are to be found in those works of Phidias which form the collection of Lord Elgin, and which afford easy access to examination. In the present corroded and tarnished state of the surfaces of these statues we cannot trace the nature or the defects of this variety, but an examination of its texture and composition in the broken fragments, serves to excite the deepest regret, that the genius of the greatest sculptor whom the world has seen, should for want of better materials have been condemned to bestow its energies on so perishable and so defective a stone. This marble is of a loose texture and moderate-sized grain, coarser than that of Carrara, but finer than that of Paros; in colour it is exceedingly imperfect, being tinged with gray, brown, and yellow, and mottled with transparent parts, which give it the appearance of having been stained with oil. But its most formidable defect is its laminated structure, and the quantity of mica with which it is contaminated : to this we are to attribute the corrosion and almost entire ruin of so many of the specimens, the action of the weather dissolving those parts of the stone where the mica is most abundant, and eating deep fissures through many parts of the work. It is peculiarly unfortunate that the two most admirable specimens, specimens which are calculated to excite in the minds of artists a mixed feeling of wonder and despair, the horse's head and the Theseus, should be those which have suffered most. Had they been fortunately executed in the more uniform and durable stone of Carrara, these works might still have been preserved to us in all their original perfection of drawing and surface. Even the hammer of the Turk would have rebounded with little injury from the marbles of this texture, while the micaceous stone of Pentelicus splitting in the direction of its laminæ, has permitted the complete mutilation of many valuable sculptures."

" We have no geological information with regard to the relations of these stones. The great resemblance of the Pentelic marble to that of Glen Tilt in aspect and composition, renders it probable that like this it lies in mica slate, forming beds parallel to and interstratified with that rock : that the others have similar relations to the primary rocks, we should have concluded on general geological principles, had we not already seen that the white marble of Sky which has given rise to this discussion belongs to the secondary strata.

" We have now to examine the white marbles which have been

discovered

discovered in our own islands, for the purpose of comparing their relative properties and the value which they are likely to possess in sculpture. I am unfortunately unable to give any account of those found in Ireland, neither having seen their places, nor being possessed of any specimens.

"That which has been found at Cape Wrath in Scotland, is of a grain much larger than even the Parian, and is consequently useless for the purpose of sculpture; and this indeed is by much the most common character of the Scottish specimens. Those of Blairgowrie, of Glenavon, and of Balahulish, are all equally characterized by this large sparry texture, and are all equally unfit for sculpture, however applicable to the purposes of architecture. The marble of Iona has been long since exhausted, and consequently requires no particular notice: however valuable from the purity of its colour and compactness of its texture, yet the uncertainty of its splintery fracture before the chisel, (that tool without which no spirited work was ever finished,) combined with its great hardness, would probably have rendered it useless in the arts, even if it were still to be procured.

" In a paper on Assynt I have already described the white marble of that district: it is of a very close texture, and although it contains no earth but lime, is of unusual specific gravity and hardness. It is incapable of being polished, a circumstance, it is true, of no consequence in statuary, since the polish only gives a false light to the surface and is not admitted of in modern sculpture; but it labours under the concomitant disadvantage of want of transparency, producing nearly the same dead effect and dry outline as is seen in a plaster cast, a fault in itself sufficient to prevent it from ever being adopted as a good material in the arts: its extreme hardness also renders it very expensive to work.

" The marble of Sky, the more immediate object of this discussion, is of a pure white colour, and appears sufficiently extensive and continuous to be capable of yielding large blocks. The purity of its colour is seldom contaminated; its fracture is granular and splintery, and its texture fine, less fine than that of Iona, but more so than that of Assynt: its compactness, hardness, and gravity, are greater than those of the marble of Carrara, which it in fact resembles in little else than colour. It is apparently well fitted for all purposes of sculpture, as it can be wrought in any direction, and has sufficient transparency, while at the same time it takes even a better polish than is required for statuary. With these good qualities, however, is combined an uncertainty arising from its unequal hardness. While some parts of the stone are nearly as easy to work as that of Carrara, many other specimens turn out so hard as to add a charge of near 50 per cent. to the cost of working: this appears to arise from the influence of
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syenitic and trap veins which traverse it, as I have before mentioned, but which however produce no change in its chemical composition, nor any other effect than that of induration. This addition of price to the current charge of working is sufficient in the harder specimens to counterbalance in a great degree the superior cheapness of the material, and the advantages derived from lower freight duty and insurance.· Such are the difficulties which oppose the introduction of the most perfect marble which has yet been found in Britain, difficulties which, slight as they are, ought, together with the prevalence of established habits and of a commercial routine, to check the extravagant hopes which have been entertained in this country of superseding by its own produce the importation of foreign statuary marble. But it will not be rendering justice to the marble of Sky if I do not add, that it possesses a property not found in that of Carrara, and one of considerable importance, at least in small sculptures. This is, that compactness of texture by which it resists the bruise which so often takes place in marble at the point where the chisel stops, an effect known to sculptors by the technical term *stunning*, and of which the result is a disagreeable opaque white mark, generally in the very place where the deepest shadow is wanted.

"I have little to add respecting the marble of Glen Tilt, as I have spoken of it in another place. Except the somewhat larger size of its grain, it is scarcely to be distinguished from the Pentelic: in colour it is precisely similar; but as the character and defects of the Pentelic, which I have already given, are equally applicable to this variety, we may fairly abandon all hope of rendering it useful in the art of sculpture."

Mr. Phillips's paper On the Oxyd of Uranium, and his drawings of the varieties of the primitive crystal and of its various modifications, forty-seven in number, made from well defined crystals found on specimens from various mines in Cornwall, exhibit a degree of patient and minute and accurate investigation of which but few would be found capable.

· Dr. Berger's and Mr. Conybeare's papers on Ireland, with the accompanying maps and sections, are most elaborate and highly interesting. Plate 9 shews in a very satisfactory manner the geological connexion between the W. of Scotland and the NE. of Ireland. Some facts connected with the courses of certain basaltic or whin dykes are remarkable. The chalk which is frequently traversed by them often undergoes a remarkable alteration near the point of contact. This change extends eight or ten feet from the dyke. Next to it the chalk is changed into a dark brown crystalline limestone, the crystals running in flakes

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as large as those of coarse primitive limestone. The next state is saccharine, then fine grained and arenaceous; a compact variety having a porcellaneous aspect and blueish-gray colour succeeds; this towards the outer edge becomes yellowish white, and insensibly graduates into the unaltered chalk. The flints in the altered chalk usually assume a gray yellowish colour: the altered chalk is highly phosphorescent when subjected to heat. In the neighbourhood of Glenarm, where a singular compound dyke, consisting of three branches, traverses the chalk, the included masses have been changed into granular marble. Other instances, of this change of the chalk are quoted, as also of changes effected by the whin dykes on rocks which they traverse, as of red sandstone to hornstone, of the slate clay of the coal-measures to flinty slate, and of the coal itself to cinders. The inference which Mr. Conybeare would draw from these facts, " were it allowable to speculate on subjects so remote from actual observation," is, " that the hypothesis which ascribes the formation of the floetz-trap rocks to submarine volcanoes, which were active at a very remote period before the seas and continents had assumed their present relative level, is both in itself more consistent, and in its application to the actual phænomena more satisfactory than any other.

Professor Hailstone's paper and Mr. Aikin's, though both short, will prove interesting to geologists. Dr. MacCulloch's on the Geology of Glen Tilt presents several facts deserving of much attention respecting stratification and the necessity of distinguishing more precisely than has hitherto been done, the different species of rocks. But our limits prevent our giving more particulars respecting these papers : Mr. Horner's on the Geology of Somersetshire, and others in this interesting volume.

A Practical Essay on Chemical Reagents or Tests; illustrated by a Series of Experiments. By Fredrick Accum, Operative Chemist, &c. &c. pp. 263. By Callow, Crown-court, Soho.

When it was ascertained that many of the substances of nature are compounds of different principles, methods were successfully employed to separate those principles by chemical means ; and the name of *Reagents* or *Tests* was given to those substances by means of which their analysis was accomplished.

To those who study chemistry, whether from motives of profit, and for general information, a knowledge of the action of these tests is absolutely necessary; indeed it constitutes one of the most objects to which the attention of the chemist ought to be directed. The practical application of chemical tests requires comparatively but little skill, and no costly ap-

                                                            paratus

paratus and instruments; but the phænomena which these bodies produce form an assemblage of facts, the knowledge of which is of infinite service, and absolutely necessary to the successful practice of the science.

Mr. Accum's treatise is drawn up with a view to facilitate the study of this department of operative chemistry. His aim has been to exhibit *experimentally* to the young chemist, a summary view of the general nature of chemical tests, with the effects and phænomena which are produced by the action of these bodies —the particular uses to which they may be applied in the various pursuits of chemical science, and the practical means, or art of applying them successfully. To accomplish this object, the author has in the first place stated separately, and in the synthetic form of propositions, the charactistic powers of each individual test, and has then exhibited a series of apposite experiments, to illustrate its attributes and peculiar mode of action, so as to interest the mind of the operator and to imprint the specific powers of the test under considertion on his memory. The young chemist may therefore easily convince himself of the action of each test enumerated in the treatise, by repeating in his own closet the experiments pointed out by the author. To accomplish this object with facility, Mr. Accum has chosen such particular experiments only as are easy to be performed, and the exhibition of which requires no other agents than those enumerated in the work, together with a small table lamp-furnace, a few test tubes, two or three evaporating basons, flasks, &c.* And as the science of chemistry affords numerous instances which render it necessary that the substances used as tests should be applied with particular care, the precautions to be observed, to guard against deceitful appearances that may occur under certain circumstances (and without which chemical tests are of little utility), are carefully pointed out by the author of this treatise, to put the experimenter on his guard, to deduce the effects produced from their true causes, and to apply tests successfully in the pursuits of analysis.

A list of all those substances for which there exist any appropriate tests has also been added; together with direct references to those reagents, by means of which the substances exhibited in that list may individually be detected.

From what we have so far stated, it will be obvious that the aim of the author, in composing this treatise, has been to furnish instructions to enable the student of chemistry to apply chemical tests to original investigations, or to enable him to perform a

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* A collection of chemical tests fitted up in a portable case, for performing the experiments described in the treatise, may be had as a companion to the work.

series of easy, amusing, and instructive experiments, in illustration of the affinities of bodies, and most important chemical changes of which certain substances of nature are susceptible in their reciprocal action.

Mr. Accum's name as an author and practical philosophical chemist is sufficiently known, and we think this treatise (which was hitherto a desideratum in chemical libraries) will add to his reputation. The work is clear and explicit. The peculiar properties of the individual tests, together with their attributes, modes of action, and the rules for applying them, are detailed with precision, and the series of experiments are well calculated to answer the intended purpose.

· A scientific and interesting work on Mental Derangement and the Treatment of Lunatics, will shortly be laid before the public, by a Medical Gentleman, who has devoted many years to the investigation of madhouses and the cure of diseases of the brain.

Dr. Duppa has just published, from an original manuscript, the late Dr. Johnson's Diary of a Tour in North Wales.

An elaborate work on the Forms of the Cranium has recently been imported from Germany, with the following title :—" Cephalogenesis sive Capitis Ossei Structura, Formatio, et Significatio, per omnes Animalium Classes, Familias, Genera et Ætates, digesta; Tabulis illustrata Authore J. B. Spix." This work contains some very good folio plates of sculls, &c. It is published at Munich.

Dr. Reade has just published, at Cork, " Experimental Outlines for a new Theory of Colours, Light, and Vision," in one volume 8vo.

Dr. Halliday has lately published, at Edinburgh, " A Letter to Lord Binning, on the subject of Lunatic Asylums ;" in which he exposes several flagrant abuses. He has been engaged lately in viewing houses for insane persons, in company with Dr. Spurzheim, in Scotland.

A small work also (which has excited considerable interest) has made its appearance at Edinburgh, entitled " Prospectus of the Anatomical Views of Drs. Gall and Spurzheim, confronted with the Edinburgh Review (No. 49. June 1815, Art. X.) and Dr. Gordon's Opinions in his System of Human Anatomy and Surgery, vol. 1. Edinburgh, 1815." By J. G. Spurzheim, M.D.

The Editors of the new edition of the Greek Thesaurus of Stephens have lately purchased the valuable Lexicographic Manuscripts of Professor Schaefer, which will be a valuable addition to the work.

LXII. *In-*

LXII. *Intelligence and Miscellaneous Articles.*

STEAM ENGINES IN CORNWALL.

By Messrs. Leans' Report for September, the average work of 28 engines, on the old construction, was 20,672,934 pounds of water lifted one foot high with each bushel of coals consumed.

The work of Woolf's engine at Wheal Vor for the same month, was 44,033,831 pounds; and of his engine at Wheal Abraham 51,348,291 pounds, lifted one foot high with each bushel of coals—the load 15 lib 2 oz. per inch in cylinder. His other engine at the latter mine lifted 22,477,408 pounds with each bushel, her load being as yet only 3·1 per inch in cylinder.

In the same month an engine is reported at Dalcouth to have lifted 40,749,286 pounds with a load of nine pounds per square inch, and one at Wheal Chance 44,354,000 pounds with a load of 12·6 per square inch, with each bushel of coals. We conclude, though this is not avowed, that Woolf's principle of using steam above the temperature of 212°, and allowing it to expand, has been in some way applied to these engines; and we do so because Mr. Watt has often declared that the maximum of the best engines on his, which is now the old, principle, is 29 millions of pounds lifted one foot high with each bushel of coals; and we know that his statement cannot be controverted.

SOLAR ECLIPSE, NOVEMBER 19, 1816.

To Mr. Tilloch.

SIR,—As the eclipse of the sun, which takes place on the 19th of the ensuing month, will be the greatest that will happen in this part of the world for many years, it will naturally excite considerable attention. It may be necessary however to inform your astronomical readers, that the first impression of the moon on the sun's disc will be 20° from the vertex of the sun, on the right hand; and not 59° as stated in the Nautical Almanac. This remark is of importance to those who are desirous of observing accurately the time and progress of the eclipse. The visible conjunction is also inaccurately stated in the same Almanac, since it takes place full one hour sooner than there set down. It is no great recommendation to that work, to know that all the elements of this eclipse are more correctly stated in *Moore's* Almanac.

I am, sir, your obedient servant,

Oct. 26, 1816.   ASTRONOMICUS.

M. Gay-Lussac has published in the number of the *Annales de Chimie et Physique* for May last, the following interesting observations "on the alteration which sulphuric ether undergoes." I kept for nearly two years sulphuric ether in a glass bottle which was half full and which was occasionally opened. I had purified this ether, by washing it several times with water, then by leaving it fifteen days on quick-lime, the whole surface of which it did not cover; and finally, by distilling it in the water-bath, and collecting only the first portions. Thus prepared, it began to boil at 35·66°: its density at 24·8° was 0·7119, and it had no action upon turnsole. Wishing lately to make some experiments on the dilatation of this liquid, I endeavoured to ascertain if it had undergone any alteration; and I found that its density had increased, that it feebly reddened turnsole, and that its boiling point, little above 35·6° at the commencement of the distillation, was removed from it, towards the end, more than 20°. Surprised at these results, I examined the residue, which was unfortunately very weak, and I recognized the following properties: its smell announced the presence of sulphuric ether and acetic ether: it was strongly acid, and united with water in all proportions:—its taste was excessively hot and acrid. A portion of this residue, to which I added a little potash, was evaporated to dryness, and the sulphuric acid afterwards sent off from it very poignant vapours of acetic acid. On another portion of the residue I poured concentrated sulphuric acid, and immediately there was separated a very limpid colourless oil, a little heavier than water, and which remained fluid at the common temperature, and the taste of which, although very hot, had not the same characters with that of the residue. I had a globule of it of the size of a large pin's head; and although I had recourse to the assistance of M. Robiquet, I could not discover if it had any resemblance with the sweet oil of wine.

On a third portion of the residue of the distillation of the ether I poured a little hydrochloric acid, and added concentrated sulphuric acid:—upon cooling it, which I accelerated by plunging the vessel into cold water, the liquid precipitated white flakes, although its density was far superior to that of the water. Having raised the temperature, the flakes were dissolved and united in a globule which had the appearance of melted wax: upon cooling, the globule became solid and preserved a little transparency: its texture was fibrous, but it had very little consistency....

This substance, evidently different from the oil which I have mentioned, has very remarkable properties. It has a peculiarly etherated smell; its taste, which is only gradually developed, is very hot, but less than that of the residue of the ether: it melts at about 65°;

65°; it is volatilized by means of water, and crystallizes in small prisms, which grow generally at right angles: its density is superior to that of water. When thrown upon a hot silver spoon, it is volatilized, leaving a black spot adhering. The sulphuric ether dissolves it very easily, and abandons it by evaporation; water seems to dissolve a small quantity of it, for it assumes the smell which is peculiar to this substance. Wishing to ascertain at the same time how a high temperature would act upon it, and if it contained chlorine, I placed the small globule which remained, at the bottom of a glass tube; I placed thereupon fragments of glass, and upon these fragments some very pure barytes. I began by heating the barytes, then the glass, and finally, the globule: immediately there was a violent detonation, which broke the tube into a thousand pieces, and the report of which resembled the smack of a whip. I could not ascertain by this experiment if the new substance contained chlorine ; for the products of the explosion did not extend to the barytes. Having discovered a small parcel of the substance in the vessel in which I had kept it, I added a little potash, which has the property of dissolving it ; I heated it, keeping the vessel well closed; I saturated the potash with nitric acid, and I added nitrate of silver. A slight precipitate was soon formed; but I operated upon too little matter to be entirely convinced of the presence of chlorine or of the hydrochloric acid in the new substance, although the circumstances of its formation render it extremely probable that it is composed of hydrochloric acid and the oil which I have mentioned above, or of chlorine and a peculiar vegetable substance.

The above observations prove at least that the sulphuric ether, when left a long time to itself, yields also acetic acid, perhaps alcohol and an oil. I shall not venture, however, to affirm that this oil had no resemblance with the sweet oil of wine, and that it had not pre-existed in the ether ; for it is very certain that the best prepared ether and the most recent leaves a very sensible speck on the glass on which we place some drops to evaporate:—in all cases the new etherated combination which I have described is very remarkable, and deserves to be better studied. I recommend the subject therefore to some abler chemists, who have opportunities of preparing great quantities of sulphuric ether.

We are concerned to learn that Mr. Singer is compelled to discontinue his public lectures, in consequence of severe illness, resulting from the rupture of a blood-vessel of the lungs.—Mr. Singer's extensive collection of instruments will, we understand, be shortly submitted to public sale, in consequence of this event.

To James Dawson, of No. 63, Strand, for certain improved means of producing or communicating motion in or unto bodies, either wholly or in part, surrounded by water or air, or either of them, by the reaction of suitable apparatus upon the said water or air, or upon both of them.—14th March, 1816.— 6 months.

To Samuel Nock, of Fleet-street, London, for an improvement in the pan of the locks of guns and fire-arms.—12th Aug. —2 months.

To Edward Biggs, of Birmingham, for certain improvements in or on the machinery, used in the making or manufacturing of pans and stails of various kinds.—14th August.—6 months.

To Robert Tripp, of the city of Bristol, for his improved hussar garter with elastic springs and fastenings, and also elastic springs and fastenings for pantaloons and other articles.—14th August.—6 months.

To William Moult, of Bedford, Esq. for certain improvements on his former patent for an improved method of acting upon machinery, bearing date the 23d day of May 1814.—14th August. —6 months.

To James Neville, of Wellington-street, Northampton-square, for certain new and improved methods of generating and creating or applying power by means of steam or other fluids, elastic or non-elastic, for driving or working all kinds of machinery, (including the steam-engines now in use) and which are applicable also to the condensing of steam and other aqueous vapours in distillation or evaporation, and are useful in various manufactories and operations where heat is employed as an agent, or where the saving of fuel is desirable.—14th August.— 2 months.

To Jean Samuel Panly, of Queen-street, Brompton, for a machine for ascertaining in an improved manner the weight of any article.—14th August.—2 months.

To Anthony Gilchrist, of Worship-square, for a machine for making of nails, screws, and the working all metallic substances. —15th August.—2 months.

To Robert Salmon, of Woburn, for his improved instruments for complaints in the urethra and bladder.—19th August. —2 months.

To John Barton, of Silver-street, for certain improvements in pistols.—31st August.—6 months.

To Charles Lacy of Nottingham, and John Lindley of Loughborough, lace manufacturers, for machinery to be incorporated with, added to, and used with parts of certain machines already

already in use, for the making and manufacturing of lace-net called bobbin or Buckinghamshire lace-net, by the aid of which the said lace-net may be made with greater facility and less manual labour than by any machinery or method now in use and known.—30th September.—2 months.

To Jacob Metcalf, of Great Mary-le-bone-street, brush-manufacturer, for his tapered hair or head brush.—30th September.—2 months.

To Robert Clayton, of Dublin, artist, for his new method of preparing, making and finishing metal and composition blocks, plates, rollers, and types and dies, by which various patterns, devices and compositions, can be effectually imprinted and impressed upon cotton, linen, silk, worsted, mohair, and woollen cloths, or any fabric made of a mixture of any two or more of them; also on paper, leather, porcelain and earthenware, at a considerable less expense than by any other process now used for these purposes.—30th September.—2 months.

To John Aston Wilkes, of Birmingham, glass-manufacturer, for his method of manufacturing glass icicles, spangles, and every description of ornamental glass, with a loop or loops of the same material.—30th September.—2 months.

To Joseph Kirkman, of Broad-street, St. James's, pianoforte-maker, for his improved method of applying an octave stop to pianofortes.—14th October.

Meteorological Observations kept at Walthamstow, Essex, from September 15 *to October* 15, 1816.

[Between the Hours of Seven and Nine A.M.]

Date. Therm. Barom. Wind.
September

15	59	30·30	SE.—Very hot sunny day; star-light night.
16	53	30·30	E.—Fine hot day; star-light.
17	52	30·50	E.—Hazy and clouds; hot sunny day; star-light.
18	55	29·90	E.—Hazy; very dark, and low clouds; slight rain 5 P.M.; dark and slight rain.
19	53	30·50	N.—Gray morning; sun and clouds; star-light.
20	55	30·10	E.—Gray; sun and clouds; bright star-light.
21	52	29·75	E.—Gray; slight showers; dark night.—New moon.
22	57	29·70	E.—Sun and hazy; hot day; clear star-light.
23	51	29·80	N.—Cloudy; hot fine sunny day; dark night.

Hour. Therm. Barom. Wind.

September

24   56   29·89   E.—Sun and clouds; at noon very black *nimbus*, NW; hazy; clouds and sun; dark night.

25   56   30·09   SE.—Gray and hazy; sun and clouds; warm day; star-light.

26   46   30·10   N.—Foggy morning; sunshine; dark night.

27   53   30·20   N.—Sun and clouds; sunny day; star-light.

28   58   30·10   N—S.—Hazy; windy and clouds; rain from 10 A.M. to 4 P.M.; star-light and showery. Moon, first quarter.

29   51   29·81   S.—Rain till about 11 AM.; high wind and clouds; clear and clouds.

30   43   29·70   E.—Sun and hazy; fine day; cloudy.

October

1   52   29·58   SE.—Very rainy; sun and clouds; moon-light.

2   56   29·64   S.—Fog; rainy and wind; showers and sun after 1 P.M.; cloudy and slight rain.

3   48   29·90   N—S.—Sun and hazy; cloudy; rain.

4   58   29·90   S.—Foggy; showers and sun; slight rain, and very damp.

5   56   29·90   SE.—Cloudy; sun and hazy; fine day; very slight rain.

6   57   29·80   E.—Sun and hazy; rain frequent after 3 P.M.; slight rain. Full moon.

7   58   29·85   E.—Rainy all day; slight rain.

8   57   29·90   SE.NW.S.—Damp and clouds; gleams of sun and hazy; fine day; moon-light.

9   58   30·00   SE.E.—Gray; sun and clouds; light but cloudy.

10   58   30·00   S.—Gray; sun after noon; light, but no stars visible.

11   56   29·97   SE.—Sun and cloudy; gray fine day; stars and *cumuli*.

12   49   30·20   E.—Foggy; clouds, and some sun; light night.

13   —   30·15   SE.—Gray day; some sun; star-light.

14   46   30·10   SE.—Foggy; sun, and *cumuli*; dark.— Moon, last quarter.

15   58   30·10   SE.—Foggy; gray day; star-light.

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1816.	Age of the Moon.	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Sept.15	23	66·	30·15	Very fine ⎫
16	24	69·	30·19	Ditto ⎪
17	25	62·	30·07	Cloudy ⎬ No rain
18	26	60·	30·12	Ditto ⎪
19	27	59·	30·29	Fine ⎪
20	28	56·	30·29	Very fine ⎭
21	new	57·5	29·83	Fair—rain in the evening
22	1	57·	29·85	Ditto
23	2	57·5	29·95	Ditto—rain P.M.
24	3	60·	29·94	Showery
25	4	60·	30·23	Fog till noon, cleared up and very fine—heavy rain in the evening·
26	5	59·	30·27	Ditto Ditto
27	6	58·	30·33	Very fine
28	7	58·	30·05	Rain
29	8	55·	29·53	Heavy rain—violent gale at night from the S.W.
30	9	53·5	29·79	Showery
Oct. 1	10	58·	29·40	Heavy rain till the evening
2	11	61·	29·58	Rainy
3	12	55·	30·08	Fair
4	13	55.	29·97	Rain
5	14	58·	30·	Showery
6	full	63·5	30·	Fine—rain in the evening
7	16	55·	30·10	Heavy rain
8	17	58·	30·12	Fair—slight showers P. M.
9	18	57·	30·25	Showery
10	19	61·5	30·20	Fair—rain in the evening
11	20	60·	30·15	Ditto—but cloudy
12	21	58·	30·27	Ditto—ditto
13	22	56·	30·22	Showery
14	23	59·5	30·20	Fine

There has been much more rain this month than the last. The corn all out yet, and a large proportion of it spoiled. The latter end of the month, there has been hardly a breeze.

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For *October* 1816.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.			
Sept. 27	55	63	55	30·18	39	Fair
28	55	61	54	29·93	0	Rain
29	56	60	51	·84	0	Stormy
30	47	62	55	·7	36	Fair
Oct. 1	55	64	55	·39	0	Stormy
2	55	61	55	·36	0	Rain
3	50	55	54	·30	0	Rain
4	55	61	50	·79	0	Rain
5	56	62	57	·79	29	Showery
6	55	64	55	·79	26	Do. with Thunder [in the night
7	56	58	55	·78	0	Rain
8	57	61	56	·92	26	Fair
9	56	60	57	·93	32	Fair
10	57	62	56	·91	38	Fair
11	55	59	55	·95	21	Showery
12	50	56	50	30·02	32	Fair
13	50	55	51	·01	27	Cloudy
14	45	60	50	·02	36	Fair
15	45	58	52	·02	39	Fair
16	47	57	50	29·85	41	Fair
17	48	55	50	·70	0	Rain
18	46	52	49	·71	36	Fair
19	47	54	46	·69	21	Cloudy
20	46	49	39	·58	25	Stormy
21	42	51	42	·60	33	Fair
22	46	52	40	·72	36	Fair
23	37	50	42	·88	39	Fair
24	46	52	45	·62	27	Cloudy
25	47	50	44	·30	0	Rain
26	43	51	48	·50	29	Fair

N.B. The Barometer's height is taken at one o'clock.

LXIII. *On Planetary Influences on the Atmosphere. By the Rev. T. D▮▮MMOND.*

To Mr. Tilloch.

SIR, — MANKIND has from a very remote period exhibited two classes of characters,—the credulous and the incredulous. It has frequently occurred that credulity has predominated without evidence or argument to give it sanction, and that incredulity has been cherished without any attempt to investigate the truth or falsehood of the positions in question.

The credulous and the incredulous have perhaps in no instance been more tenacious in opinion than on the subject of planetary influence. As the object of this paper is confined to the consideration of planetary influences on the atmosphere, it is not necessary for us to investigate the ground on which philosophers of preceding periods contended for the universal prevalence of starry influences.

The hypothesis I am disposed to advocate is of great antiquity ; we can trace its vestiges in remote periods—its origin baffles all chronological precision.

The doctrine of planetary influence was the foundation of the Sabian system of religious worship. It engaged the attention of Babylonian and Egyptian sages; and in every civilized state of human existence, as amongst the Perses, the Hindoos, and the Chinese—people whose antiquity distances all competition—we find that astrological science was accounted worthy the attention of the most intelligent persons. Modern philosophers have limited the title of science to astronomy, *i. e.* the elementary part; and consigned astrology, *i. e.* the *application* of elementary knowledge combining observation and inferences, to disrepute, if not to oblivion.

Indiscriminate ridicule, censure, or contempt, are not worthy of admission within the pale of philosophy.—Whilst the fabulous stories of the *Pleiades* or the *Hyades* having been the daughters of Atlas, are mere modern romances, comparatively speaking, or at best enigmatical vehicles of some concealed truth, and most probably of Grecian invention,—we must look to the extensively prevalent Sabian system of theology as far more ancient, and we may rationally infer that in its primitive reception it was not chargeable with idolatry.

The etymology of the term is uncertain,—perhaps from Sabos in Arabia*,—but Sabianism or Sabism is now generally understood to imply the worship of the heavenly bodies.

If

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* There can be little doubt, I think, about the etymology of this word. It is derived from צבא *tsaba*, commonly pronounced *saba*, an army or host.

If we resolve to reject every doctrine which has at any time been misapprehended, or whose truths have by the multitude been confounded with error, the subjects for our belief will be very limited.

The opinion adopted by the *Perses*, the *Chinese*, and the *ancient Greeks*, appears to have been, that a supreme ruling invisible Power exists, with whom the planets and stars are so connected as to be the creative and productive powers, the immediate agents of the Deity. Thence the Chinese of the ancient school, at this day worship *Tien* or Theaven, by which they understand the supreme power presiding in that vast expanse where the planets and stars have their respective motions. From the positions of the planets, from the rising and setting of the constellations, they have time immemorial attempted to predict the vicissitudes of the seasons; and since every change in the atmosphere must be the effect of some secondary operating cause, it is at least decorous to investigate before we condemn a practice which has received the testimony of ages in its defence.

The Sabian system of worship, which enrols the planets and stars in its ritual as objects of adoration, is to be considered one of the many instances in which error has tarnished the lustre of truth.

If we refer to the history of the patriarchal families, we find the primitive religion in its purity;—in the records of the Old Testament we perceive that the patriarchs retained those correct principles of true religion, which acknowledged the unity and supremacy of the Deity, and limited religious homage to that one God, whilst the planetary bodies and all secondary causes were regarded merely as the ministers of the will of God. —An extract from Josephus may serve to testify that the idea is correct with regard to Abraham:

" Abraham was the first * that adventured to preach up the doctrine of one GOD, the Almighty Maker and Creator of all things in heaven and earth: and that for all the comforts we enjoy in this world, it is to his infinite goodness, not to any power in ourselves, that we stand indebted for them. This he argued from the orderly course of things, both at sea and land, in their times and seasons, and from his observations upon the *motions* and *influences* of the *sun, moon,* and *stars;* insomuch that, without an over-ruling and an administering providence to keep the whole a-going, the whole frame of the universe must drop into confusion; and consequently that all we have to trust

host.   In the sacred volume the sun, moon, and stars are called the *saba,* that is, *the host,* of heaven (Deut. iv. 19); and the Israelites were expressly commanded not to worship the *saba,* but him who made them, and who is therefore called JEHOVAH צבאות *subaoth*—the Lord of Hosts.—EDIT.

i. e. amongst the Chaldeans of that period.

                                                                    to,

to, for matters either of pleasure, profit, or necessity, depends singly upon the good-will and bounty of the first mover; so that it is to him alone that we are to render all honour and thanks-giving, without assuming any thing to ourselves."—L'Estrange's Josephus, chap. viii.

The Book of Job is a fair specimen of Sabian theology before the less refined sentiments of what is now denominated Sabian worship were introduced.—The beautiful poem, with the argu-mentative narratives contained in the Book of Job, is of great an-tiquity;—no internal evidence assists us in determining the age in which it was written, nor the author to whom we are indebted for a composition which has merited, and will ever merit, the ad-miration of mankind. The personification of the agent of evil is a specimen of poetical licence, but the subsequent matter is simple in its theology and correct in its ethics. The sublimity of thought and the pure morality contained in this work are admitted to be superior to the productions of Homer.

Although many allusions are made to the customs of patri-archal ages, or rather of the early stages of civilization, it may not be without hesitation inferred that the Book of Job was fa-bricated beyond the precincts of improved civilization and highly cultivated talent.—The arguments of the friends of Job are founded on principles similar to those which have in several periods of the world formed the bases of academic questions.

I have no wish, sir, to fill your pages with theological or ethical discussions, nor to involve your readers with investiga-tions concerning the probable writer of that excellent composi-tion,—whether Enoch, or any of his predecessors or cotempo-raries, is of no moment,—the state of astronomical, and, if the distinction must be observed, of astrological science, at the æra in which the poem was written, is deducible from the mention of *Mazaroth, Arcturus, Orion,* and the *Pleiades,* more espe-cially from the mention made of the sweet *influences* of the *Pleiades;* whence we must infer that the writer was no stranger to the prevalent opinion of sidereal influence.

What the writer of the Book of Job appears to have credited, —what Abraham from the statements in Josephus appears to have corroborated,—may, I beg leave to presume, be considered as having constituted a part of that knowledge which Moses acquired amongst the Egyptians, and in which Daniel and his associates became proficients in the court of Babylon.

Impelled by a desire of attaining the knowledge of truth, and with that view desirous of divesting myself of all prejudice, I must avow that I see *nothing* in a creed considering the starry host as the agents of Deity by their mutual attractions and repulsions, at the same time limiting to the *Divine Being* all the reverence

X 2 of

of worship which Jewish prophets may not be supposed to have adopted, and to which Christian divines may not be allowed to subscribe. Regarding those astrological atmospherical observations which have escaped the ravages of time, as the fragments of a science which was successfully cultivated in many ancient periods, I presume that the attention of the moderns directed to the changes in the atmosphere, and to the coincident situation of the planets, may improve our meteorological knowledge, and enable us to recover all that was ever known by the sages of antiquity.

The sun is generally allowed to be the exciter of what we call heat in the atmosphere and on our earth; and it is not requisite on this occasion to inquire whether the sun emits light and heat, or whether it excites them in the body of the air and the surface of the earth.

The moon is commonly supposed to have no atmosphere like our own; but that it has an influence on whatever constitutes our atmosphere is generally acknowledged.

The influences of the sun and moon are so well authenticated by the observances on the periodical flow and ebb of the tides in the waters encompassing our earth, that no differences in opinion exist relative to the ordinary causes and effects; and it may not be unworthy our attention to inquire, whether extraordinary ebbings and flowings are not imputable to other planetary and sidereal influences.

No one questions that what is usually expressed by the term attraction operates between bodies of different magnitudes; and confining our attention to what is denominated the solar system, whatever properties or qualities are ordained by the divine Creator to regulate the movements and limit the approximation of the planetary bodies, must be conceived to operate in every part of the space in which they revolve.

It is of no moment in the present consideration how far the late Rev. J. Wesley was correct in his remark, that astronomers assuming the *magnitudes* of the heavenly bodies infer their *distances,* and assuming their *distances* infer their *magnitudes:* the question is, what plausible reason can be assigned whence we may infer that their relative situations produce those atmospherical changes, " *the skiey influences*" to which we are subjected.

A small ball freely suspended near the side of a mountain is by an undiscernible influence moved from a perpendicular direction,—a small magnet has a property of producing an effect not only at a distance through the intervening portion of air, but of manifesting its operation, although *glass, wood,* and a variety of other substances, intervene;—yet it is by many regarded as incredible that such large bodies as Jupiter and Saturn can

<div align="right">have</div>

have any corresponding action or effect. The planetary bodies, according to modern philosophers, are too remote to act on each other, or on the atmosphere around us. The varying belts of Jupiter and the extended ring of Saturn may be atmospheres, yet not perfectly similar to the aërial envelope surrounding our earth; but every planet must be surrounded with a fluid, whatever its peculiar gaseous quality; and *in* and *through* that fluid the planet, whether by *mechanical* or *chemical attraction* or *repulsion*, must be conceived to *rarefy*, *condense*, or *otherwise* operate. As the moon is capable of transmitting its influence through the surrounding medium to the waters and to the earth which compose our globe, we may infer that each planet has a certain sphere of operation: and however we may suppose the energy of that operation to decrease as the distances increase, or whatever may be the intervening medium, we must allow *planetary* is as capable as *lunar* influence, of exerting its proportion of agency through that medium.

<div align="center">Yours respectfully,</div>

Priory, Gray Friars, Norwich,   T. DRUMMOND.
    Nov. 7, 1816.

LXIV. *Some Observations on the Salt Mines of Cardona, made during a Tour in Spain, in the Summer of* 1814. *By* THOMAS STEWART TRAILL, *M.D. Member of the Geological Society* *.

THESE celebrated mines occupy the head of a small valley in the immediate vicinity of Cardona, a town in the province of Catalonia.

This valley extends about half a mile in length, from the river Cardonero to the mines, in a direction from east-south-east to west-north-west. Its north western side is bounded by a very steep and lofty ridge, the summit of which is crowned by the town and castle of Cardona. The opposite boundary is somewhat less elevated; but both sides are considerably higher than the upper surface of the fossil salt. On entering this valley, the attention is arrested by bold cliffs of a greyish-white colour, which are soon discovered to consist of one vast mass of salt. The sides and bottom of the valley are composed of reddish-brown clay, forming a thick bed, from which here and there large imbedded masses of rock salt project in the manner of more ordinary rocks; especially along the winding ascent which leads up to the town of Cardona. The summits of the ridges which bound

            the.

the valley on each side, are formed of a yellowish grey sandstone of a coarse texture, and containing many scales of grey mica.

The great body of the salt forms a rugged precipice, which is reckoned between 400 and 500 feet in height at the upper extremity of the valley, and is covered by a thick bed of the clay above mentioned.

The precipitous form is partly owing to the manner in which the mine has been wrought for a series of ages. There is no excavation; but the salt has been procured by working down perpendicularly as in an open quarry. The lowest part of the present works has a solid floor of pure salt which is not above the level of the bottom of the valley where no salt is found; but the real depth of the bed of salt has never yet been ascertained. The upper surface of the salt is not level; but appears irregularly elevated, according to the general outline of the hill in which it occurs.

The salt has been usually represented as forming an entire mountain: but though it here appears supplying the place of common rock, yet from its being confined to this valley, and not attaining so high a level as the surrounding hills, it would seem more correct to consider it as a mass or bed of salt filling up a valley, than as constituting a mountain, which according to some authors* is a league in circumference. These dimensions could only be obtained by considering the neighbouring heights as formed of this mineral; a supposition not countenanced by my personal observation, nor by the best information which I could collect on the spot.

The surfaces of the salt precipice which have been long exposed to the weather are not smooth, but cut into innumerable shallow channels, running in a tortuous manner, and divided from each other by thin edges, often so sharp as to cut the hands like broken glass. The channeled surface is evidently produced by the action of the winter rains, which have given the whole a striking resemblance to the surface of a mass of ice, which had been partially thawed and again frozen.

The general colour of the exposed surface is greyish white, with here and there a tinge of pale reddish brown, from the colouring matter of the superincumbent bed of clay. Towards the extremities of the mass of salt, extremely thin layers of a pure and plastic clay are insinuated between layers of salt, so as to give it the waved delineations which often occur in some species of calcsinter. The general mass of salt is however of the greatest

* *Introduccion à la Historia Natural y à la Geografia Fisica de España, por Don Guillermo Bowles.—*Madrid, 1775.—Dillon, who translates him. *Labord, Itineraire descriptif,* &c.

purity;

purity; and in order to be converted into snow white culinary salt requires no other process but grinding. The greyish hue of the external surface is owing to the rain penetrating a portion of the salt, and by diminishing its opacity, depriving it of the white-ness which the fresh fracture generally presents. At the period of my visit the surface of this immense mass was perfectly dry, and in some places, where water had most recently flowed, was covered with a snow-white efflorescence. This circumstance, as well as the sharpness of the edges above mentioned, show the little hygrometric water in the atmosphere of that country, and the general purity of the salt from earthy muriates.

The fracture of the salt is highly crystalline, and usually ex-hibits large granular distinct concretions, which give it some-times the appearance of a breccia, or of containing imbedded crystals.

A perennial brine spring flows at the foot of the great preci-pice, and affords a strong proof of the little effect of water on this very compact salt. The aperture through which the stream has issued for many years, is not wider externally than two feet, and suddenly contracts to a few inches; while the channel worn in a solid floor of salt, through which the stream has long flowed, is not a foot in depth. This is partly to be ascribed to the water being saturated with salt; but during the rainy season the stream is much augmented, and thus cannot be supposed so highly charged with saline matter. Notwithstanding this, neither the solvent nor mechanical effects of the spring seem to have much effect on the fossil salt of Cardona. The waters of this spring flow into the Cardonero, leaving in the valley a thick scaly crust of salt, resembling the ice formed around our brooks in similar situations. During the rainy season, it is asserted that the stream carries down such quantities of salt into the Cardonero as to kill the fish in that river. This assertion rests upon the authority of Bowles, an able naturalist; but he un-doubtedly was led into error when he asserted, that the waters of the Cardonero at some leagues below the mines, yield no trace of salt; from which he inferred, that salt may, *by motion*, be converted into earthy matter. At Manresa, which is about twenty miles below Cardona, I tested the water of the Cardonero by nitrate of silver, which indicated the presence of an unusually large portion of muriate of soda. The taste of the brine spring at Cardona is intensely saline; and the hand immersed in it, on being exposed to the air, is instantly covered with a film of salt. The salt rock near its source is most elegantly veined with deli-cate waved delineations of an ochre yellow colour.

The clay which covers the bed of salt at Cardona and forms the sides of the valley, exactly resembles the clay found in the

salt,

salt district of Cheshire, having when dry some resemblance to shale but becoming plastic when moistened. It is remarkably pure, and free from intermixture except of salt, large masses of which are occasionally imbedded in it.

No fibrous salt was to be observed at Cardona; nor did I discover the slightest trace of gypsum in that neighbourhood; a remark which was also made by Bowles. On the soil near the town, a small quantity of a saline efflorescence was however observed, which had the taste of sulphate of soda; but the loss of the specimen I collected, has prevented a more accurate investigation of its properties.

The salt mine of Cardona is wrought like an open quarry with pickaxes and wedges, by which the mineral is raised in considerable tabular masses. The part at present wrought presents an extensive horizontal floor of pure rock salt; the level of which is a little lower than the foot of the great salt precipice. An enormous mass of the same mineral lies between this precipice and the present mine, the removal of which will, in time, render the appearance of this interesting spot still more magnificent; for then the vast front of the rock salt bed will at once strike the eye from the lowest part of the mine.

Like every other public work in Spain, the mines of Cardona are in a languid state from the effects of the late war which has desolated the peninsula. Only two labourers are at present employed in quarrying the salt, and in wheeling it to the receiving house. Over these, eight overseers are appointed, who do duty in rotation; and ten sentinels are continually stationed around the mine to defend it from the depredations of the peasantry. Several clerks are employed in an office built at the entrance to the mine, and the whole is under the direction of an *intendente* or inspector, who wears the uniform of an officer in the Spanish army; for the mine is the property of the crown, and is most rigidly guarded. Notwithstanding the rigour with which depredators are punished, the peasantry frequently attempt to deceive the vigilance of the guardians of the mine. When detected, the usual punishment for a peasant is, even on the first offence, two or three years labour among malefactors in some of the public works in the province. A soldier is however less severely punished when he commits a similar transgression; he is generally sentenced to a few days solitary confinement in a dungeon of the castle. On asking an overseer the reason of this disproportion in the punishment of different offenders, he replied, that the soldier's poverty was supposed to extenuate his crime, while the peasant of Catalonia enjoyed comparative wealth, and could afford to purchase salt for the consumption of his family.

Such

Such is the boldness of the smugglers and the jealousy of the government, that it is dangerous to visit the mines without formal leave from the intendente; as the sentinels have orders to fire on any one seen loitering about them.

The workmen here receive considerable wages, and are all free labourers; each man receives daily twelve reals vellon, which at the rate of exchange last year equals three shillings sterling; lads are paid at the rate of eight reals, or two shillings; and boys receive six reals, or one shilling and sixpence *. The hours for work are from six in the morning to seven in the evening (in summer); with the intervals of half an hour, between eight and nine o'clock A.M., for breakfast, and two hours, from twelve to two, for dinner, and its usual sequel in Spain, the *siesta.*

The produce of the mines is pulverized by grinding it in mills, on the exact construction of our common water mills. This operation reduces it into an excellent culinary salt of a snowy whiteness. In this state it is sold to the peasantry of the surrounding districts, at the rate of thirty reals vellon, or seven sh. six d. sterl per fanega of five arrobas of Catalonia, which equal 116 pounds avoirdupois.

As there are no roads practicable for wheel carriages in this part of Catalonia, the salt is carried from Cardona on mules or asses; the only beasts of burden that could travel in safety the rugged defiles in which this district abounds. It seems a part of the perverse policy of the Spanish government to discourage the formation of proper roads, lest it should facilitate the operations of the smuggler.

It would not be difficult to connect Cardona, by means of a canal, with the ocean: and thus the valuable produce of its salt mines might increase the revenues of the crown, and the trade of Barcelona. The channels of the Cardonero and Lobregat always contain a large body of water, and might easily be rendered subservient to the purposes of inland navigation. Besides augmenting the value of the mines of Cardona, such a plan, by facilitating the intercourse with the interior of this fine province, would stimulate the exertions of a people who only require an equitable government to become highly industrious.

It yet remains that I offer a few remarks on the nature of the country around Cardona, as materials for its geology.

Its general appearance is mountainous. The mountains are abrupt, but generally wooded. The valleys are narrow, and,

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* This may be considered as liberal wages where the necessaries of life, with the exception of bread, are cheap; at Cardona, mutton and beef cost one real vell. per twelve ounces. Bread of the best quality costs one real vell. per twelve ounces. Wine of the country (a very good red sort) is retailed at six quartos per bottle, or about two-pence sterling.

where

where the declivities will permit cultivation, they produce abundance of good grapes and some corn. In coming from Barcelona, the traveller leaves, at a small distance on the left, the majestic Montserrat; and gradually approaches a mountain chain proceeding from its northern extremity, which declines as it stretches towards Manresa. This chain consists of similar materials to Montserrat; viz. of vast beds of farcilite, composed of rounded masses of quartz, with angular pieces of siliceous slate, and fragments of clayslate united by a basis containing calcareous earth. The fragments of this farcilite become smaller as we go northward, and at last bear a striking resemblance to coarse greywacké; to which formation I am inclined to assign the puddingstone of Montserrat, and the chain of which it forms a part *.

On descending the rugged mountains of puddingstone into the valley of the Lobregat, before coming to Manresa, we observe strata of a blueish-grey rock with interposed layers of a softer material of the same colour, which crumbles into sandy clay by exposure to the weather. These strata have some resemblance to sandstone-flag; but an attentive consideration convinced me that they ought to be considered as stratified greywacké approaching to greywacké slate. Above these we again find the farcilite, which is the prevailing rock about Manresa. All the rocks hitherto mentioned effervesce slightly with acids; a circumstance which connects them in some measure with the extensive limestone country to the south-west of Montserrat; and they all show a tendency to split vertically into columnar masses. Beyond Manresa the farcilite occurs till the traveller crosses the ford of the Cardonero, when it is succeeded by a limestone of a dirty iron brown colour, and dull, almost earthy, fracture. Beyond the village of Suria, a sandstone, which slightly effervesces with acids, makes its appearance. This rock constitutes the sides of the valley which contain the fossil salt.

The immediate vicinity of the salt mines shows no other rock than a yellowish grey sandstone much charged with scales of mica.

We find thus that the salt rock of Cardona is accompanied by clay and sandstone, like our Cheshire salt formation. Limestone also is found near it; but the usual concomitant gypsum

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* It may not be improper here to remark, that the common descriptions of Montserrat are in several respects erroneous. It is *not an insulated mountain*, as generally represented; but is the highest point of a considerable chain. Its insular appearance, as seen from the high road between Igualada and Martorel, has deceived those who have never examined its north-eastern side. The *touchstone*, mentioned by Bowles and others, as entering into the composition of its puddingstone, appears by its fracture to be only a dark coloured common siliceous slate.

<div align="right">appears</div>

appears to be wanting, as well as fœtid limestone. The great compactness and purity of this salt merit examination.

Though the country around Cardona is mountainous and rugged, it is inferior in elevation to the districts between it and the Mediterranean; as well as to those which bound it on the north. Immediately behind Cardona the mountains begin to ascend with increasing boldness until they unite with the grand chain of the Pyrenees.

I relinquish to others the difficult task of giving a probable explanation of the formation of rock salt; contented if my observations on the mine of Cardona can add any thing to the mass of facts which should guide us in the obscure but captivating speculations of geology.

LXV. *Observations on some Combinations of Azote with Oxygen*. Read to the French Academy of Sciences on the 9th of Sept.* 1815. *By M.* Dulong.

CHEMISTRY sometimes exhibits combinations so difficult to isolate, and the production of which is accompanied by circumstances so complicated, that the most expert and accurate observers obtain a knowledge of their properties only after long efforts and successive labours, in which in a manner they expunge all kinds of errors before attaining the truth. Among those mysterious compounds we may reckon the combination of azote and oxygen long known under the appellation of *vapeur rutilante (nitreus vapour,* or *nitrous acid gas).* Notwithstanding the numerous researches on this subject, it is only since M. Gay Lussac's last experiments that the true proportions of it have been known. I had myself made some experiments on the same subject; and as my results differ in several respects from those obtained by M. Gay Lussac, I shall submit them to the opinion of the public.

When we distil neutral nitrate of lead previously dried, we obtain a very volatile liquid of an orange yellow, which had already been remarked by M. Berzelius in his researches upon the composition of the nitrates; but which was examined more particularly by M. Gay Lussac, whose researches yielded as a result that this liquid ought to be considered as the acid of the nitrites, in which the elements are kept in combination by the action of the water. The existence of water in the dried nitrate of lead, the proportions of which correspond to those of the nitrates perfectly deprived of water, will present a very sin-

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gular exception to the laws of the composition of salts; and as it is rather by analogy than by direct experiments that Gay Lussac was led to adopt this opinion, I have endeavoured to subject this liquid to a rigorous analysis.

We may effect our purpose by placing copper or iron, for instance, in contact with the acid in vapour at a red heat. In order to avoid the errors which might result from the action of the acid on the corks employed in the common apparatus, I had at first made use of a glass apparatus, all the parts of which were soldered; so that the acid could only be in contact with the glass and the metal which ought to compose it: but I met with difficulties during the experiment, which made me recur to the employment of a porcelain tube with corks. In consequence of some precautions which it is needless to mention here, the only error which can result from it is confined to the absorption of a very small quantity of acid by the cork; every thing being so arranged that nothing could be extricated which could complicate the products of the operation.

The iron or copper in very fine and well polished wires was employed in great excess, and all the oxygen of the acid was absorbed by the metal; the gas not absorbed afterwards passed through a tube of muriate of lime before proceeding under the bell-glass which terminated the apparatus. It is easy to see that by determining the weight of the metal, that of the tube of muriate of lime, before and after the experiment, and the volume of the gas liberated, we might attain the utmost precision. I have made by this process several experiments, the results of which have not presented sensible differences.

In one of those analyses the azotic gas was pure, or at least the quantity of hydrogen which it could contain was below the limits prescribed by the usual means for detecting it. In general the proportion of hydrogen was always extremely small. The following are the details of the experiment in which the proportion of this gas was the greatest, and in which, nevertheless, it formed only the thirty-two thousandth part of the volume of the azote:

Weight of the acid analysed ..	..	7·935 grs.
Increase of weight in the iron	..	5·660
Do. of muriate of lime ..	..	0·017
Volume of dry gas at 0° B. 0·76ᵐ	..	1·96 lb.
100 parts of this gas contain hydrogen ..		3·22 parts.

This proportion of hydrogen corresponds to a quantity of water which united to that absorbed by the muriate of lime will only form six thousandth parts of the weight of the acid analysed.

As this quantity is far inferior to the smallest proportion of water which we can admit as the essential element of a combination,

bination, it appears to me that it ought to be ascribed to the
humidity of the air, or to that which remains attached to the
vessels, and which it is impossible entirely to get rid of in mani-
pulations which are a little complicated. Besides, this acid is
not exhibited under the orange colour, but when it is anhy-
drated ; for I have remarked that the smallest quantity of water
is sufficient to render it green, as will be explained a little lower
down. The liquid acid obtained from the distillation of the
nitrate of lead does not therefore contain water, nor does
the nitrate of lead dried contain any. On calculating the
ratio of the azote with the oxygen in the liquid acid according
to the above results, we find that 100 parts of azote are com-
bined with 233·8 of oxygen. These proportions differ very little
from those which we may deduce for the composition of the
nitrous acid gas from the ratio in point of volume which M.
Gay Lussac has given ; for we find, on setting out from this
ratio, for 100 of azote, 228 of oxygen. The identity of the ni-
trous acid gas and of the liquid acid obtained by the distillation
of the nitrate of lead seemed to me at first so extraordinary, that
I thought some error had slipped into the weights ; but after
having repeated several times the same experiment, with results
very little different, there did not remain a doubt as to this iden-
tity. It then became very evident, that the nitrous acid gas
could not be a permanent gas. In order to verify this inference,
I made the following experiment :

I placed in two cylindrical reservoirs two bell-glasses with
cocks, one containing nitrous gas and the other oxygen gas: by
means of two funnels with stop-cocks, in which water was kept
constantly at the same level, I regulated the flowing of this liquid
into the two reservoirs, so that the quantity of nitrous gas dis-
placed by the water in one of the bell-glasses was a little less
than the double of the oxygen extricated, during the same time,
from the other bell-glass; both gases severally passed through
a long tube partly filled with muriate of lime and partly with
quick lime, and afterwards met in a tube of a larger calibre con-
taining fragments of porcelain: by this arrangement, the gases
mixing perfectly, they are transformed into nitrous vapour al-
most pure, and merely containing a small excess of oxygen. The
gaseous mixture afterwards passed into a curved tube, submitted
to an artificial cold of 20° below zero. After having passed
some litres of gas over into this apparatus, I obtained in the
cooled tube a slightly greenish liquid, sending out into the air
very abundant yellow vapours, and which was transformed during
decantation into an orange yellow liquid possessing all the pro-
perties of that which proceeds from the distillation of the nitrate
of lead. This experiment does not allow of any doubt, and we

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ought to admit that the compound of azote and oxygen which is generally known by the name of *nitrous acid gas*, is not a gas at the common temperature and pressure of the atmosphere, but rather a liquid which I shall call in the meantime *dry* or *anhydrous nitrous acid*, to avoid all misunderstanding.

I found 1·451 for its specific gravity, and 28° for the temperature at which it enters into ebullition, the barometer being at 0·76ᵐ.

If we have hitherto mistaken the physical properties of this combination, it is because the vapour which it forms maintains a very strong tension at the common temperature; and in the greater number of cases in which it is produced, it is mixed with permanent gases which prevent its condensation.

We may easily see from this, that the condensation of the dry nitrous acid will be the more difficult, and that it will be necessary to employ, in order to produce it, a temperature so much the lower as the proportion of foreign gas is greater. This accounts for the differences which are observed in the products of the distillation of the nitrates. When the base of the salt has but a feeble affinity for the acid, and when it allows it to be extricated at a low temperature, the nitric acid is decomposed only into oxygen and nitrous acid; and even if we shall suppose that these two bodies are set free at the same time, the vapour of the nitrous acid forming at least the two-thirds of the gaseous mixture, it might be partly condensed, even at the temperature of 15°: this is what happens with the nitrate of lead. When on the contrary the base forcibly retains the acid, and precipitates, the having recourse to a very high temperature for the decomposition of the salt, the greater part of the nitric acid being then reduced into oxygen and azote, it will require a considerable degree of cold to liquefy even in part the nitrous acid. Thus by submitting the gases which are liberated during the decomposition of the nitrate of barytes to a cold of 20° below 0°, I have not obtained a single drop of liquid; because, as we know, the greater part of the nitric acid was then transformed into a mixture of oxygen and azote.

It is also for this reason, that when it is our object to make directly with oxygen and nitrous gas dry nitrous acid, we must so manage as to leave but a feeble excess of oxygen, as I have indicated above; otherwise we shall obtain but very little liquid.

If we pass into the apparatus described a little higher a mixture of nitrous gas and oxygen gas, in which there is a little more than four parts of the former to one of the latter, there is still condensed a liquid in the cooled tube: but this liquid is of a very deep green, and much more volatile than the foregoing. I

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have analysed this liquid by the process which I have described above for the analysis of dry nitrous acid; and I found that it was formed of 100 parts of azote and 207 parts of oxygen, in weight, in one experiment; in another, I found 216 parts of oxygen for the same quantity of azote. It is evident, besides, from the way in which this liquid has been formed, that water does not enter into its composition. The proportion of oxygen which it contains is less than that of the nitrous acid; but it is greater than that which ought to be found in the acid of the *nitrites*, which Gay Lussac has denominated *pernitrous*.

It is probable, according to this, that it is not a homogeneous combination, but a simple mixture of dry nitrous acid, and another compound of nitrous and oxygen gas, in which the proportion of nitrous gas will be much stronger, and this conjecture is also supported by the manner in which this liquid acts during its distillation. In fact, by submitting it to a gentle heat the green colour gradually becomes weaker as the volatilization is effected, and there remains a variable quantity of dry nitrous acid. If the green liquid is not a simple solution of nitrous gas in dry nitrous acid, it ought to contain another combination of nitrous gas and oxygen, probably in the proportions of the pernitrous acid, which it will perhaps be possible to insulate by reiterated distillations, since there exists a difference, although in truth very feeble, between the temperatures of ebullition of the green and the orange liquid. This is a question which I shall endeavour to decide on account of its importance, notwithstanding the difficulties by which it is surrounded. When we place dry nitrous acid in contact with a great quantity of water, by immediately shaking the mixture it is instantaneously decomposed; a proportion of nitrous gas is extricated, which varies according to the rapidity with which the decomposition is effected. If we put, on the contrary, a very small quantity of water with the same acid, no gas is disengaged; but the acid becomes a very deep green: this is what happens when we pour, drop by drop, dry nitrous acid into any mass of water; because this acid being heavier than water, it gains the bottom of the vessel, undergoing the change of colour just mentioned. It is evident, from what has been said, that the conversion of the orange acid into green acid, in this circumstance, ought to be ascribed to the decomposition of a part of the dry nitrous acid into nitric acid which is dissolved in the water, and in nitrous gas which is combined with the remains of the non-decomposed acid. Lastly, if we mix successively with a certain quantity of water various portions of nitrous acid, the extrication of nitrous gas produced by the same weight of acid will always diminish, until it ceases entirely, although the liquid continues to absorb nitrous acid. We

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then see the water successively become a greenish blue, a dark green, and an orange-yellow. These variations of colour are the same with those which have been so long observed in the nitric acid of different degrees of concentration, when we pass into it nitrous gas of variable proportions. It seems, therefore, that this gas acts, in this last case, by bringing back a portion of nitric acid to the state of nitrous acid, and passing itself to this state. As to the nitric acid which is found mixed in very different proportions with these various combinations of oxygen and azote, according to the manner in which they have been obtained, it does not seem to affect their colours sensibly; at all events, it will not be correct to suppose that these mixed acids are only simple mixtures; for the nitrous acid seems to have an affinity for nitric acid, if we judge according to the recent observations of Mr. Davy, on the properties of the nitro-muriatic acid.

The anhydrated nitrous acid, when placed in contact with a strong solution of potash, is decomposed ; nitrous gas is extricated, but in smaller quantity than when the same decomposition is produced by water:—nitrite and nitrate of potash are formed. With liquid ammonia, the action is extremely violent, and the nitrous gas extricated is mixed with azote ;—this proves that a part of the ammonia has been decomposed.

When we pass nitrous acid in vapour over dry caustic barytes placed in a tube at the common temperature, the vapour is slowly absorbed ; but at a temperature of above 200° the barytes suddenly becomes incandescent, and no elastic fluid is extricated. The combination which results melts, and is afterwards very difficult to dissolve :—we therein find once more nitrate and nitrite of barytes. It is certainly a very remarkable phænomenon, and the explanation of which is not very easy, to see the barytes transformed into nitrate and nitrite, at a temperature which seems to be very superior to that which will be necessary to decompose those two salts when once formed.

I have endeavoured to determine the action of the different combustible bodies on the nitrous acid when reduced into vapour; but all the phænomena which I have observed may easily be foreseen by the theory, and this saves me the trouble of relating them. I shall merely say, that all these experiments confirm the opinion that the nitrous acid yields, less easily than the euchlorine, the oxygen which enters into its composition: iode, for instance, may be sublimed into this vapour without receiving any alteration from it: sulphur and even phosphorus require, in order to inflame, a higher temperature than in pure oxygen.

The anhydrous nitrous acid is combined, without undergoing decomposition, with the concentrated sulphuric acid ; and it is

probable,

probable, according to the experiments of Gay Lussac, and also those which I have made known, that the crystalline substance described by Messrs. Clement and Desormes, in the memoir in which they have explained the formation of the sulphuric acid, is nothing but this combination.

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Since the above memoir was publicly read, I have observed that the nitrous acid may be presented under very different colours, according to the temperature to which it is subjected. The orange-yellow colour which I have assigned to it in the course of this paper, is that which it has at the temperature from 15 to 28: it is deeper the more it approaches its point of ebullition. At this temperature it is almost red; and we know that at an elevated temperature its vapour is of a very deep red. But below 15° the colour becomes weaker and weaker, down to 0°, then it is no longer of a fawn yellow; at —10° it is nearly colourless; and finally, at —20° it is entirely so. We may easily produce those different shades by shutting up in the bulb of a thermometer a certain quantity of dry nitrous acid, the temperature of which is lowered successively by the evaporation of ether, or carburet of sulphur. It will be curious to observe if the same liquid subjected to a temperature of —40° or —50° will be presented again under a coloured form; and if this colour will be blue, as indicated by the theory of coloured rings:— this I shall endeavour shortly to ascertain.

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LXVI. *Experiments and Observations to prove that the beneficial Effects of many Medicines are produced through the Medium of the circulating Blood, more particularly that of the Colchicum autumnale upon the Gout. By* Sir EVERARD HOME, *Bart. V.P.R.S. Communicated by the Society for improving Animal Chemistry* [*].

A KNOWLEDGE of the readiness with which liquids pass from the stomach into the circulation, carrying along with them the impregnation of different medicines; and the readiness with which such medicines are carried off from the circulating blood, by the action of the kidney, led Mr. Brande and myself to an inquiry respecting the production of gravel and gout, upon which subject he has laid two separate papers before the Society.

In these communications, the action of different substances on the contents of the stomach has been considered, and those

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* From the Transactions of the Royal Society for 1816, part ii.

substances most efficient in depriving them of the principal ingredient met with in stone and gout, are pointed out.

For the cure of gout, the *eau médicinale* of Husson has been most fortunately discovered to be a specific remedy, and it is now ascertained, by experiments on different people, that a vinous infusion of the *colchicum autumnale*, or meadow saffron, is equally so, and therefore the two medicines must be considered as the same.

To ascertain their mode of action, appeared to me an inquiry connected with the objects of this Society, which are not confined to the knowledge of purely chemical combinations in the stomach or other parts of the body, but include the effects of galvanism on the nerves, and of mineral and vegetable solutions on the blood, so far as they affect the actions of life, or the symptoms of disease.

It has already been determined by experiment, that almost every mineral, vegetable, and animal poison, if not the whole of them, is carried into the circulation before it produces its specific effects upon particular parts, whether these are the stomach, skin, or other parts of the body. The most truly specific medicine that we have been hitherto acquainted with, is mercury for the venereal disease; and it is completely established, that this remedy, when in the circulation, is equally efficient in the cure of a recent chancre produced by inoculation, and a venereal sore throat, in consequence of the disease having been carried into the circulation.

That other medicines can be received into the circulation, and, as soon as they arrive there, produce their effects upon different parts of the body, is proved by experiments made by the late Mr. Hunter, although he had no idea of their being usually carried there before they produce the different actions so well known to follow their exhibition by the mouth. He found that infusions of the following substances received into the circulation by the jugular vein, immediately produced the same effects which more slowly follow their being taken by the mouth. Infusion of opium brought on drowsiness. Infusion of ipecacuanha vomiting. Jalap vomiting and purging. Infusion of rhubarb a profuse flow of urine. These effects ceased in a few hours, and appeared to have in no respect injured the animal's health. Except the venereal disease, gout is the only one whose local symptoms have been completely removed by medicine, in so short a time, as to put it beyond all doubt that their removal is the effect of the medicine. The effect of the *eau médicinale* and of the vinous infusion of the *colchicum autumnale* on gout, is indeed more rapid than that of mercury on the venereal disease, but in all
                                                          other

other respects corresponds with it ; and if these medicines act through the medium of the circulation, the only difference may be, that the one is more quickly received into it than the other.

This power of the *eau médicinale*, which I have stated to be exactly similar to that of the *colchicum autumnale* over the local symptoms of gout, I have ascertained by experiment more than six times upon myself; at one time the symptoms went off in six hours, at another in twelve, and at others in twenty-four hours.

As we know the sensible effects of mercury, whether it is introduced into the circulation by the absorbents, or received into the stomach, are the same, we conclude, whenever these sensible effects are met with, that mercury is actually in the circulation.

It therefore occurred to me, that if the sensible effects of the infusion of the *colchicum* should prove to be the same, whether it is introduced into the circulation by the jugular vein, or received by the mouth into the stomach, that we might equally in both cases conclude it to be in the circulation. To determine this point, thirty drops of the vinous infusion of the *colchicum* (made by macerating two pounds of the fresh roots in twenty-four ounces of Sherry wine, in a gentle heat for six days, the spirit being previously carried off by heat,) was diluted with a drachm of water, and conveyed into the circulation of a moderately sized dog by the jugular vein. The dog's pulse in a natural state is 140 in a minute.

In five minutes, the dog had a tremulous motion of the muscles and fluttering of the pulse, accompanied with nausea, but no retching to vomit. In fourteen minutes, the pulse was 180 in a minute and had frequent intermissions. In four hours, the pulse was 120 in a minute, of its natural strength, and had frequent intermissions. In seven hours, the dog had a natural motion, the pulse had no intermission, was 140 in the minute. The dog had a good appetite for food, and appeared in perfect health.

The same dog at the end of three complete days swallowed sixty drops of the same infusion, exactly double the quantity that had been introduced into the circulation. In two hours, he became languid, the pulse wiry and weak, but 140 in the minute. In four hours and a half, the languor much less and the pulse natural. In eight hours, the dog had had a natural motion. In eleven hours, was in good spirits and very well.

The sensible effects upon the dog were similar to those produced upon myself, but in a less degree. Under the influence of a violent fit of the gout, in the ankle, on the 23d of December 1815, at ten o'clock in the morning, I took sixty drops of

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the *eau médicinale;* the pain of the gout was insufferable, I got into bed, and was so chilly as not to be able to keep my hands warm, even under the bed clothes. In two hours, I became rather hot and thirsty. In three hours, the pain was so much diminished as to be tolerable, while the limb was at rest. In seven hours, I had a confined motion from the bowels, and the pain in the ankle became severe, while the foot was placed on the ground, but this went off as soon as the foot was again placed in a horizontal posture. A nausea, or half sickness, came on; my pulse, which is naturally 80 in a minute, was lowered to 60, and intermitted. In ten hours, the nausea was gone off, but I remained languid, the pulse beating 70 in a minute. I had some appetite for food.

The following morning, my pulse was 80, and having passed a good night, I was enabled to walk as usual, and follow the duties of my profession.

If these observations shall be confirmed, they must lead us to conclude, that the different kinds of substances, which produce specific diseases, are first carried into the circulation, in the same manner as mineral and animal poisons, and that the medicines by which they are acted upon, go through the same course, before they produce their beneficial effects; a material step will thus be gained in the consideration of diseases, and the modes of treating them.

LXVII. *An Appendix to a Paper on the Effects of the Col-chicum autumnale on Gout. By Sir* EVERARD HOME, *Bart.* V.P.R.S.*

W HEN I laid before the Society my paper upon this subject, I was anxious to establish what appeared to me to be two important facts; one, that the infusion of the *colchicum* can be received into the circulation without producing any permanent mischief; the other, that it is through the medium of the circulation its beneficial effects upon gout are produced, and there-fore the sudden relief which is experienced can be readily explained. Having attended to the effects of the *eau médicinale* and of this medicine for several years in cases of gout, both in my own case, and in those of my friends, I found, invariably, that they diminished the frequency of the pulse, ten or twenty beats in a minute, and this effect generally took place about twelve hours after the medicine was exhibited: I therefore considered this to be the criterion of the constitution being under the influence of the medicine; and when I found that the pulse

was affected in the same way by the medicine received into the circulation, and in a much shorter time, I became satisfied that in both cases this arose from an effect upon the circulation, and not upon the stomach, and therefore did not further prosecute the inquiry; since exhibiting larger doses could only confirm what is already known, namely, that the medicine is capable, when injudiciously used, of producing very violent effects.

It has been suggested to me since the paper was read, that the only mode of proving that the medicine acts through the medium of the circulation, is to show that when a sufficient quantity is received into the blood, all the violent effects are produced, that result from a large dose taken by the mouth; and as I had no object but the pursuit of truth, I lost no time in complying with this suggestion, and introduced into the circulation of a dog 160 drops of the same infusion before employed.

The animal instantly lost all power of voluntary motion, the breathing became extremely slow, and the pulse was hardly to be felt. In ten minutes, the pulse was 84, the inspirations natural, which are 40 in a minute. In twenty minutes, the pulse was 60, the inspirations 30 in a minute, a tremulous motion had taken place in the hind legs. In an hour, the pulse was 115, and irregular; the animal was capable of sitting up, but was in a state of violent tremor, and the inspirations could not be counted.

In one hour and a half, the tremor had gone off, the pulse continued the same; the animal made ineffectual attempts to vomit, and continued to do so for ten minutes, accompanied with great languor; the inspirations were 54 in a minute.

In two hours, the pulse was 150, and very weak; the animal had voided one ounce and a half of water, had vomited twice, each time bringing up a quantity of mucus tinged with bile, and had two liquid stools.

In three hours, had vomited again, and had another stool; the pulse too weak to be counted.

In four hours, continued extremely languid.

In five hours, vomited some bloody mucus, and expired.

On opening the body, the stomach contained mucus tinged with blood, and its internal membrane was inflamed; the duodenum had its internal surface universally inflamed; the same appearance in a less degree was met with in the jejunium and ilium, and more strongly marked in the colon than in the ilium.

The facts which I have now adduced, afford sufficient proof of the action of the *colchicum autumnale* upon the different parts of the body, being through the medium of the circulation, and not in consequence of its immediate effects upon the stomach and intestines.

LXVIII. *New Outlines of Chemical Philosophy.* By EzEKIEL
WALKER, *Esq. of Lynn, Norfolk.*

[Continued from p. 245.]

THE electrical apparatus, mentioned in my last paper, consists
of about 3,300 groups of zinc, copper, and paper discs, inclosed
in six glass tubes. These are placed in an horizontal position,
each resting upon two insulating rods, which stand seven inches
high above the common base into which they are fixed. In the
manner of connecting the ends of the tubes there is nothing
new, but in the application of the silver leaf electrometer, there
is a mode which may afford some amusement to those who are
not much acquainted with physical experiments.

The wire on the top of the electrometer may be placed either
in contact with one extremity of the arrangement of columns,
as represented in the figure *, or at any more convenient di-
stance. The electrometer being placed upon a table in the
middle of a room, and the electrical apparatus in some remote
place, out of sight, it only requires a thin music wire to connect
the apparatus with the electrometer, to cause the pendulums to
vibrate as freely as if the electrometer and columns were in ac-
tual contact.

It has been supposed by M. De Luc and some other writers
on this subject, that the variable action of two electric columns
upon a pendulum suspended between them, is owing to the
moisture contained in the atmosphere; but others are of opinion
that the temperature of the air, and not moisture, is the cause of
this variation.

To determine the cause of irregularity in the vibrations of my
pendulums, I made observations several times a-day, for more
than a month, and I find that their vibrations are governed by
the temperature of the air of the place in which the apparatus
stands. The moisture of the atmosphere, I apprehend, could
produce no effect, as the ends of the glass tubes were carefully
closed with sealing-wax.

The following observations show the effect of temperature
upon electric columns:

Oct. 1816.	Therm.	No. of Vibrations of the Pendulums per minute.
10 at 9 A.M. ..	60	136
10 at 10 A.M. ..	62	140
10 at 11 A.M. ..	64	142
10 at 1 P.M. ..	67	144
10 at 3 P.M. ..	68	148
10 at 8 P.M. ..	66	144

In

In keeping a register of the variable action of M. De Luc's columns, some attention is necessary in adjusting the pendulums to the maintaining power; for if the pendulums be too long, or too heavy, their vibrations will be irregular, stopping at intervals till the columns have accumulated sufficient power to put them again into motion. If the columns be sufficiently powerful to keep heavy pendulums in regular action when the weather is cold, their vibrations will agree very nearly with the temperature of the air at all other seasons; but if the pendulums be very short, they will vibrate too quickly for observation.

The two elements which keep the pendulums in motion come from the earth; for when the electrometer is placed in contact with the copper extremity of the series of columns, no action is produced; but when a person standing on the floor lays his finger upon the zinc extremity, the pendulums will instantly begin to vibrate, and continue in motion as long as the communication with the earth shall remain in the same state. The electrometer being removed to the zinc extremity, no action will commence; but let a communication be made between the copper extremity of the series and the earth, and the pendulums will vibrate as freely as before.

Although the two elements which keep the pendulums in motion come from the earth, yet the variable state of the earth's electricity has no effect upon them, for they will make the same number of vibrations in a given time, the temperature of the air being the same, whatever the electrical state of the earth may be.

It is well known to every electrician that his machine acts much better at one time than at another. This has been attributed to various causes; but the true one is, the electricity of the earth is variable, not only on different days, but at different times in the same day.

The method that I use for determining this property of the earth, is very simple, and yet it admits of considerable accuracy. I take a barometer tube about two feet long, and after having drawn it once through a piece of silk held in my hand, I hold it over one of my phial electrometers, at the distance of about an inch.

If the leaves of the electrometer diverge and remain permanent, the angle which they contain is measured, and noted down as the electrical state of the earth at that time. If the leaves be not permanently electrified with the tube at that distance, it is brought into contact with the electrometer, when a permanent effect will generally be produced; and the angle, being measured, is entered in a column titled *contact*. But it will sometimes happen, that the earth contains so little electricity,

that

that even contact produces no permanent effect upon the electrometer. On the contrary, the electricity of the earth is sometimes so strong, that the tube drawn once through the silk as before, and held at the distance of four, five, or six inches above the electrometer, the leaves will become permanently electrified.

But to obtain a greater degree of exactness than can be derived from one electrometer only, I make use of five or six, and after having made an observation by each separately, I take a mean of their results for the true. I have kept a journal of the electricity of the earth by this mode for more than fifteen months, and found, during that time, such a correspondence among these instruments, as gives me great confidence in this mode of investigation.

Extract from my Journal of Observations on the Electricity of the Earth, taken at Nine o'Clock in the Forenoon.

1816.	By Induction	Dist in Inche	By Contact.
July 11	136	3	
19	00	1	00
21	00	1	10
24	00	1	150
28	140	1	
Oct. 13	105	1	
14	98	1	
15	90	2	
16	106	2	
17	123	2	
18	108	3	
19	124	2	

When a tube, excited as above directed, is brought into contact with an electrometer, without producing any permanent effect within it, it is not to be understood that the earth contains *no* electricity, but only that it is in a *very low state.*

The experiments, described in my last paper, demonstrate that the pendulums placed at each extremity of an arrangement of electric columns, were kept in motion by two invisible elements, and I have to add, that these pendulums have now been constantly vibrating for more than two months. In the year 1813, I ascertained that the electric spark consists of two elements of equal mechanical forces passing through each other in contrary directions[*]. In these experiments, the effect was as

* Phil. Mag. vol. xlii. p. 163.

instantaneous

instantaneous as the spark itself; but the effect of the same two elements, brought into action by the electric columns, may be continued at pleasure for months, or even years.

Now, as all the operations of Nature are produced by general laws, it is very probable, that all the phænomena of electricity, chemistry, meteorology, and the living functions of animals and vegetables, are the effects of these two elements.

This hypothesis (like many others) is easily formed, but the investigation of a theory is a work of greater labour; and may perhaps, in this instance, require the assistance of some instrument not yet invented; for nothing tends so much to enlarge the native intellectual powers of man as improvements in the arts.

Lynn, Nov. 13, 1816.   EZEKIEL WALKER.

[To be continued.]

LXIX. *On the Action of detached Leaves of Plants. By* T. A. KNIGHT, *Esq. F.R.S. In a Letter addressed to the Right Hon. Sir* JOSEPH BANKS, *Bart. G.C.B. P.R.S.* *

DEAR SIR,— SINCE I had last the honour to address a communication to you, with a request that you would lay it before the Royal Society, I have repeated great part of the experiments which formed the subjects of my former letters, with such additions and variations, as might probably lead to the detection of any erroneous conclusions which I might have drawn: but I have not been able to detect any errors, nor to add any thing very important to my former observations. I have, however, been able to ascertain a few new facts, which I think too interesting to be lost.

I endeavoured, in my former communications, to adduce evidence, that the matter, which becomes vitally united to trees, previously passes through their leaves; and I shall now proceed to state some facts, which, I trust, will prove that a fluid possessing the power which I have attributed to the true sap, actually descends through the leaf-stalks.

A slender knife was passed through some leaf-stalks of the vine, about two-thirds of an inch distant from their junction to the branch; and down to that point, the leaf-stalks were divided longitudinally, and a transverse section, about half an inch long, was made through the bark opposite the middle of the leaf-stalk. A similar transverse section through the bark, was made somewhat less than an inch distant below; and these sections were

united

united by two longitudinal sections through the bark, which ex-
tended from the extremities of the upper tranverse sections to
the extremities of the lower; by which means, pieces of bark,
about half an inch broad, and nearly an inch long, were sepa-
rated from the adjoining bark. These were then detached from
the alburnum, and surrounded by two folds of paper coated with
wax on each side; by which all connexion and communication
with the tree, except through the divided leaf-stalks, were cut
off. The insulated pieces of bark, nevertheless, continued to
grow, and extended downwards, and laterally, and in thickness;
and thin layers of alburnum were deposited.

Leaves of the potatoe, without any portion of bark being at-
tached to them, were taken from the plants, just at the period
when the tuberous roots began to be formed; and I conceived
that these leaves, consistently with my former experiments and
conclusions, must contain portions of the living organizable
matter, which would subsequently have been found in their tu-
berous roots. The leaves were, therefore, planted in pots, and
placed under glass, where, being regularly and properly supplied
with water, they continued to live till winter, though without
emitting fibrous roots; and I then expected to find some small
tubers at their bases. In this expectation I was disappointed;
but the result of the experiment was not less satisfactory, the
bases of the leaf-stalks themselves having swollen into conic
bodies of more than two inches in circumference, and being
found to consist of matter apparently similar to that which com-
poses the tuberous roots of the plant. The enlarged parts of
the leaf-stalks remained alive in the following spring; but whether
they are capable of generating buds or not, I have not been able
to ascertain.

Leaves of mint were planted in the same manner as those
above mentioned; which grew, and continued alive through the
winter, and were still living in the end of the last month, having
assumed the character of the thick fleshy leaves of evergreen
trees. Upon examining the mould in the pots, I found it to
contain very numerous roots, which must have derived their me-
dullary, and their cortical, and alburnous substances from mat-
ter which had emanated and descended from the leaves.

I had frequently observed, in former experiments, that the
destruction of the mature leaves of young plants not only sus-
pended the growth of the roots, but also the growth of the im-
mature leaves; whence I inferred, in a former communication,
that the organizable matter, which composes the young leaves,
has always undergone a previous preparation in other leaves of
the plant, either of the same, or preceding season; and I was
thence led to expect that, under favourable circumstances, the
mature

mature leaves might be made to nourish and promote the growth of immature leaves, without the aid of roots. Several shoots of the vine, each about a yard long, were detached from the trees, and laid over a succession of basins of water, into which each of the mature leaves was in part depressed: and thus circumstanced, the young leaves continued to grow, and the points of the shoots to elongate; and all were alive and in perfect apparent health at the end of a month. The water necessary to preserve the young leaves must in this case have been derived from the mature leaves; and I entertain no doubt, but that the organizable matter which occasioned their growth, was derived from the same source. Intersection of the bark between the mature and young leaves was not attended with any injurious consequences, and the sap must, therefore, have passed to the young leaves through the alburnum.

Consistently with the preceding circumstances, if the mature leaves be destroyed, or taken off, the fruit ceases to grow, or, if full grown, remains without richness or flavour; and the power of feeding fruits in winter and early spring seems to be confined to evergreen plants. The orange and lemon tree, the ivy and holly, afford familiar examples of this; and where a genus of plants consists of evergreen and deciduous species, as that of *mespilus* and *viburnum,* the evergreen species alone nourish their fruit in winter and early spring.

The probable passage of the sap from the mature to the young leaves and fruit may, I think, be easily pointed out, though decisive proof of its course will probably never be adduced. Having often detached the bark from the alburnum of the stems of young oaks, just at the period when the midsummer shoots were beginning to elongate, I observed, as others have done, that a fluid exuded from those parts of the surface of the alburnum, which are called (most improperly) the medullary processes, and from correspondent points of the bark, which resemble the medullary processes in organization. This fluid has been proved, by its power of rapidly generating an organic substance, to be the true sap of the tree, part of which I conceive at this period, to be passing from the bark to join the ascending current in the alburnum; which current feeds the young succulent shoots and growing leaves. Subjecting the alburnum to a slight degree of pressure at this period, I found that a considerable quantity of liquid, being apparently the true sap of the tree, issued out laterally through the medullary processes, as well as longitudinally through the cellular substance of the alburnum: but the tubes of it continued empty, and their position was marked by depressions of the surface of the extravasated fluid. I endeavoured to ascertain, what proportion of water a given quantity of the

alburnum of such oak trees contained at this period; and I found that 1000 parts lost by drying only 371 parts: which is not more than the weight of the water that the cellular substance appears capable of containing, entirely independent of the tubes. That the tubes, nevertheless, are not always empty, but that they act at other periods of the year as reservoirs for the sap, I have given an opinion in a former communication; and I am now in possession of facts which prove them to perform this office, even in the heart wood, to a much greater extent than I had ever at any former period suspected; and which incline me to believe, that the durability of the heart wood, as well as of the alburnum of the oak, will be found to depend to a great extent upon the period in which the tree is felled : but I propose to make my observations upon these points the subject of a future communication.

<div align="center">I am, my dear sir, &c.</div>

<div align="right">T. A. Knight,</div>

LXX. *Controversy respecting Safety-lamps.*

LAST month, but too late for insertion in our October number, we received from a respectable correspondent at Newcastle several articles which have appeared in the Newcastle Courant, claiming for Mr. Stephenson priority of invention of the safe-lamp.—When the invention was first announced, we stated our opinion that Mr. Stephenson and Sir Humphry Davy made each their discovery independent of each other. Mr. Stephenson's friends, on the contrary, have expressed an opinion that Sir Humphry's was derived from Mr. Stephenson's; and in support of their opinion the dates of his experiments have been brought forward by himself. Sir H. Davy's friends, resting also on dates, have shown that Sir H. was prior, Of course, if dates are to be taken as evidence, not merely of priority of invention, but as proof that the one who was latest must have borrowed, it will follow that Mr. Stephenson's lamp was derived from Sir Humphry's.—The following are the articles which have appeared on this subject in the newspaper to which we have alluded.

From the Newcastle Courant of the 19th of October.

I trust the public, and gentlemen of the coal trade in particular, will excuse my requesting their attention to a letter from Robert William Brandling, esq. to the meeting summoned for the purpose of voting a piece of plate to Sir Humphry Davy, and to a resolution which he moved at a subsequent meeting, and which was seconded by Arthur Mowbray, esq. Whether or not

not the former of these gentlemen is justified in the opinion he
has expressed, and which he has kindly allowed me to publish,
it appears to me may easily be decided; and I shall only add,
that if it can be proved I took advantage in the formation of
the safety-lamp, of any suggestions except the printed opinions
of scientific men, I deserve to lose the confidence of my honour-
able employers, and the good opinion of my fellow-men, which
I feel an honest pride in declaring, even in my humble situation
in life, is of more value in my estimation than any reward that
generous but indiscriminating affluence can bestow.

<div style="text-align:right">GEORGE STEPHENSON.</div>

<div style="text-align:center">*Copy.*</div>

<div style="text-align:right">" Low Gosforth, Aug. 29, 1816.</div>

Sir,—As it will not be in my power to attend the meeting
summoned for the purpose of voting a piece of plate to Sir Hum-
phry Davy for the invention of the safety-lamp, I trust the gen-
tlemen will excuse my adopting this mode of requesting them to
ascertain (previous to their resolving upon a measure which will
convey their decided opinion to the public) whether the merit of
that invaluable discovery is due to that gentleman, or not. The
conviction upon my mind is, that Mr. George Stephenson, of
Killingworth Colliery, was the person who first discovered and
applied the principle upon which lamps may be constructed, so
as to be used with perfect safety in mines charged with hydro-
gen gas ; whether that gas is admitted through capillary tubes,
or the apertures of wire-gauze, (which may be considered as
merely the orifices of capillary tubes) does not, I conceive, affect
the principle. In the communications I have seen from Sir
H. Davy, no dates are mentioned, and it is by a reference to
them only that the question can be fairly decided ; for the in-
formation of the meeting, therefore, I shall take the liberty of
inclosing some I received from Mr. Stephenson, to the correct-
ness of which, as far as I am concerned, I can bear testimony.
At the same time I must beg leave to add, that the principle of
admitting the hydrogen gas only in such small detached portions
that it would be consumed by combustion, was, I understand,
stated by him to several gentlemen as the idea he had embraced
two months before this lamp was actually constructed.

<div style="text-align:center">I remain, sir,</div>

<div style="text-align:right">Your very obedient humble servant,</div>

<div style="text-align:right">ROBERT WILLIAM BRANDLING.</div>

To the Secretary of the General
Meeting of the Coal Trade.

The Killingworth lamp with one tube to admit the air, and a
slide at the bottom of the tube to regulate the quantity of air
<div style="text-align:right">admitted,</div>

admitted, was first tried in the Killingworth A. and B. pits on Saturday, October 21, 1815; but being found not to burn well, another was ordered the same day, with three capillary tubes to admit the air, and tried in the mine on the 4th of November following, and found to burn considerably better, and to be perfectly safe. On the 17th of November it was tried at Killingworth Office with inflammable air, before Richard Lambert, esq.; and on the 24th of November before R. W. Brandling, esq., C. J. Brandling, esq. and Mr. Murray.

On the 30th of November a lamp was tried in the mine, in which the air was admitted by means of a double row of small perforations, and found to be perfectly safe and burn extremely well; and on the 5th of December it was tried with inflammable air, before the Literary and Philosophical Society of Newcastle-upon-Tyne.

N. B. The lamp which was tried on the 21st of October was a considerable time in making (a month at least), owing to the necessity of having the glass made and well tempered before the lamp could be begun to be made.

At an adjourned Meeting of Coal-owners, held the 11th of October 1816, John George Lambton, esq. M.P. in the chair, Mr. William Brandling moved (seconded by Mr. Arthur Mowbray), that the meeting do adjourn until, by a comparison of dates and an inquiry into facts, it shall be ascertained whether the merit of the invention of the safety-lamp is due to Sir H. Davy or George Stephenson; and the question being put thereon, the same passed in the negative.

To the Editor of the Newcastle Courant.

High Heworth, October 21, 1816.

Sir,—In answer to the letter and certificates of Mr. Stephenson, respecting the Killingworth lamp, in your last week's paper, I will thank you to insert the following notices respecting the progressive views and experiments of Sir H. Davy, on the subject of his safety-lamp.

On the 28th of August 1815, Sir H. Davy told Mr. Fenwick, of Dipton, at Auckland Castle, that he intended introducing a lamp into the coal mines incapable of inflaming the atmosphere surrounding it, and constructed of materials not easily injured.

Sir H. continued in the north of England till the 29th of September following, on which day he wrote to me, requesting me to send him a quantity of fire-damp from a blower. In this letter he says, " I have thought a good deal on the prevention of explosions from fire-damp, and I entertain strong hopes of being able to effect something satisfactory on the subject."

On the 13th of October, he acknowleged the receipt of the
fire-

fire-damp, and said, " My experiments are going on successfully, and I hope in a few days to send you an account of them. I am going to be fortunate far beyond my expectation."

In a letter, dated 19th of October 1815, he says " I have already discovered that explosive mixtures of mine-damp will not pass through small apertures or tubes, and that if a lamp or lanthorn be made air-tight on the sides and furnished with apertures to admit the air, it will not communicate flame to the outward atmosphere."

On the 25th of October he announced his discoveries to the Chemical Club of London ; but he soon " gave up the idea of a safety-lamp dependent upon a diminished circulation of air, because he found that apertures in the top and bottom only, were not safe, unless made so small as to occasion a great loss of light in the flame: *i. e.* 1-50th or 1-60th of an inch ; and he adopted tubes and canals *above* and below, which proved to be safe by direct experiment." In a letter dated October 30, he describes a lamp of this kind, and has the following reasoning upon it: " Atmospheric air, when rendered impure by the combustion of a candle, but in which the candle will *still burn*, will not explode the gas from the mines ; and when a lamp or candle is made to burn in a close vessel, having apertures only above and below, an explosive mixture of gas merely enlarges the light, and gradually extinguishes it without explosion. Again, the gas mixed in any proportion with common air, I have discovered will not explode in a small tube, the diameter of which is less than 1-8th of an inch, or even a larger tube, if there is a mechanical force urging the gas through the tube." Some manuscript copies of this letter were taken by professional men of this neighbourhood on the 2d of November ; and on the 6th of that month I read it to a general meeting of the coal trade.

On the 4th of November, Mr. Butler, in an oration delivered at the foundation of the College of the London Institution, noticed the discovery the following manner:—" At the instant I am now speaking science is advancing towards us with an invention which to the latest posterity will prove incalculably beneficial to humanity in general and commerce in particular. You have read in your newspapers of the horrid effects of the firing of a mine. A very recent paper has given an account of such a disaster. Now within these few weeks, one of those men, *homines centenarii*, as Scaliger called them, who exist but once in a century, men who elevate the country in which they were born, and even the age in which they live, our illustrious countryman, Sir H. Davy, has discovered a process by which this evil principle in nature is absolutely subdued, and all possibility of danger from it altogether removed."

The

The regular progress of Sir H's investigations after this time till he discovered a *tissue* permeable to *light* and *air*, but impermeable to *flame*, may be seen in his publications on the subject.

In a letter dated from Whitehaven Castle, 20th of September 1816, Sir H. pointed out to me a little book, entitled " Elements of Chemical Science," by J. Murray, Lecturer on Chemistry*, dated June 1815, in which there are several hints for constructing a lamp upon the principle of a diminished atmosphere. " A lanthorn might be made air tight, and fed through a flexible tube, &c." " A division in the tube, or another parallel to it, would promote a proper current, &c. &c." Mr. Stephenson's first lamp, which dates its origin from October 21, 1815, was upon the same principle; so was that of Dr. John Murray, of Edinburgh, announced in November 1815; and that of R. W. Brandling, esq. with the bellows on its top, which appeared soon after.

From the preceding statement, compared with Mr. Stephenson's dates (and even taking into the account the anxious two months after " he had embraced the *idea*,") it is evident he was preceded by Mr. Murray, in the application of one tube, and by Sir H. Davy, in numerous apertures and tubes above and below, for " admitting the hydrogen gas, only in such small detached portions, that it would be consumed by combustion."

I believe Mr. Stephenson to be a very modest and a very ingenious man; and that the *first* ideas he had of his lamp were the effect of his own reflections on the subject: but when it is insinuated, as it has been, that hints respecting the Killingworth lamp were clandestinely smuggled to Sir H. Davy, which led him to the invention of the wire-gauze safety-lamp, the calumny upon his character is not to be borne. The scientific world are highly indebted to him for his late discoveries respecting the nature and properties of flame; and the part of the population of this neighbourhood connected with the coal trade, owe him a mighty debt of gratitude for the successful application of these discoveries, to lighting the coal-mines cheaply and securely. He has, I know, spent a year of great anxiety and labour in the service of the coal-owners; it is, therefore, impossible to suppress one's indignation, on hearing that doubts respecting his claims to these discoveries should have arisen among that body of men, especially when his enemies in the scientific world have been held in silence by their astonishment at the novelty and originality of the invention.

I cannot conclude this letter without observing that I have

* This gentleman has been one of the warmest advocates for Sir H. Davy's lamps.

not

not yet seen a lamp, excepting the wire-gauze one of Sir H. Davy, which in my opinion deserves the name of a *safety-lamp;* and that this is the decided opinion of persons interested in the coal trade, is evident from the wire-gauze lamp being used in exclusion of every other kind, in almost every colliery in this country, in which fire-damp prevails.

<div style="text-align:center">I am, sir,</div>

<div style="text-align:center">Your very obedient humble servant,</div>

<div style="text-align:right">JOHN HODGSON.</div>

LXXI. *On Vision. By Mr.* W. PATER.

To Mr. Tilloch.

SIR, — MR. HORN, in his Observations on the Cosmogony of Moses, has been so obliging as to give his ideas on the seat of vision, concluding "that the optic images are formed by caustic reflection and exhibited in the middle of the vitreous humour, and thus the optic impression and position of the tangible object are reconciled." I will confess I do not exactly understand what is meant by caustic reflection, and therefore shall make no remarks upon it; nor should I have particularly noticed the other part of the opinion, had it not brought to my recollection a correspondence I held with an acquaintance of mine, about five-and-forty years ago, who had formed an idea that the eye sees objects placed before it without any medium being necessary, and that light merely renders bodies visible by diffusion over their surfaces, he having no notion whatever that the light must pass from the surface, or point illuminated, to the eye, to produce vision. Now it appears to me that Mr. Horn's theory is founded upon some such idea, otherwise where is the necessity of supposing that the rays are reflected from the nerve to form images in the middle of the vitreous humour? because, if images were formed in the vitreous humour, which seems contrary to the nature of transparent fluids, still the rays of light must be reflected *back again* from those images to the optic nerve to produce vision, because *vision* is a sensation produced by the *action of light upon a nerve, or nerves,* adapted to receive excitement from light. No part of the body is possessed of sensibility but what has nerves, or is nervous,—the *nerves alone* being *sentient;* consequently were light capable of being collected in the transparent vitreous humour so as to form images in its centre, still, those images could not be perceived there unless the vitreous humour were nervous, which is not the case; and therefore, if these images were formed there, the light must be re-

flected

flected back again to the optic nerve to produce vision, or sen‑
sation, as nothing but nerves is sensible.

It appears to me, then, that Mr. Horn's theory demands two
reflections which are neither proved, nor necessary, nor con‑
sistent with the nature of transparent fluids; and therefore I
feel inclined to retain my old opinion, that *light* acts upon the
optic nerve and *excites sensation*.

I know the difficulty of getting rid of notions which have be‑
come habitual, and therefore I suppose my want of conversion
is owing to my having long thought as I do, and to Mr. Horn's
reasons and reflections not being sufficiently caustic to reach *my
case*, and produce in me a new and juster way of thinking: at
the same time I by no means expect that my opinions will pro‑
duce any change in Mr. Horn's ideas: however, there can be
no harm in diversity of opinions on doubtful subjects, if good‑
humour be president. I am, sir,

<div align="right">Your most obedient servant,</div>

Skiff-haven, Nov. 1, 1816.   W. PATER.

LXXII. *On the Possibility of alloying Iron with Manganese.
By* DAVID MUSHET, *Esq. of Coleford, Forest of Dean* *.

I HAVE in your last number shown the difficulty of combining,
to any material extent, metallic manganese with cast iron, by
fusing the latter with the black oxide of manganese and certain
proportions of charcoal. I next attempted to form the alloy of
the two metals by fusing certain proportions of the ore of each
metal in mixture, considering that results obtained under such
a mode of operation, would indicate the practicability of working,
if necessary or advantageous, ores of manganese along with the
ordinary ores of iron smelted in our blast furnaces, either for
the production of good bar iron or steel.

I selected a large piece of argillaceous iron ore, which I pre‑
pared by roasting and subsequent pulverization; I then passed it
through a small wire sieve: the oxide of manganese and char‑
coal were prepared in a similar way, and the whole kept shut
up from access to atmospheric air, to prevent as much as possi‑
ble any irregularity in the results by the absorption of moisture.
The crucibles and lids were accurately ground and fitted to each
other, and entrely free (by being previously baked) of any coaly
of extraneous matter.

No. 1. Fused of the argillaceous iron ore . . .   500 grs.
                          charcoal 1-5th, or 100

A crystallized metallic button was the result of this fusion,

<div align="right">weighing</div>

weighing 223 grains, or 44¼ per cent.; the earthy matter of the ore had resolved itself into a brownish opaque glass, partially transparent in thin fragments.

No. 2.   Argillaceous iron ore ..   ..   ..   500 grs.
               Oxide of manganese   100
               Charcoal   100

Result of the fusion of this mixture, a rough irregularly crystallized button of metal, weighing 217 grains, which is a produce of 43$\frac{4}{10}$ per cent. Glass opaque blackish brown without any transparency.

No. 3.   Argillaceous iron ore ..   ..   ..   500 grs.
               Oxide of manganese from which 22 per
                 cent. of oxygen and moisture had been
                 extracted ..   ..   100 grs.
               Charcoal   100

Result—A smooth regularly crystallized metallic button, weighing 230 grains, equal to 46 per cent. On a comparative examination of the fractures of these experiments, the following remarks were made.

No. 1 button possessed a clear metallic fracture inclining to steel, filed and cut soft, and possessed considerable tenacity. The grain was flat, confused like steel approaching to cast-iron. No. 2 broke with a dark gray blue glance, more perfectly granular than the former, cut softer at first, but possessed less real tenacity. No. 3 was extremely brittle, and disparted on the first application of the chisel:—fracture different from the former, silvery gray, crystalline distinct small grain, and resembled on the whole some varieties of gray cast-iron. This difference of appearance and increase of weight may probably with justice be attributed to a small portion of metallic manganese being reduced from the roasted oxide. The glass of the first button was cloudy opaque, though in thin fragments transparent. That of the second button entirely opaque : but the glass of the third button with the de-oxidated manganese was amber green, thoroughly transparent in thin pieces.

The following experiments were performed with an enlarged dose of charcoal, to compare results with the former.

No. 4.   Argillaceous iron ore ..   ..   ..   500 grs.
               Charcoal   125

The fusion of this mixture yielded a perfect crystallized button of iron weighing 257 grains, equal to 51¼ per cent. Glass lead-milky-blue, tinged: eight grains of charcoal mixed with some brilliant specks of carburet of iron were found unacted upon. This fusion was so perfect, and the metallic reduction so entire, that there did not seem to remain in the glass any appreciable quantity of iron. The addition of 25 grains of charcoal

coal in this experiment beyond that in No. 1, revived 34 grains of additional produce, and in every respect effected a complete reduction of the ore.

No. 5.   Argillaceous iron ore .. .. .. 500 grs.
            Oxide of manganese 100
            Charcoal ¼th of the ore 125

A rough metallic button resulted from the fusion of this mixture, which with some small globules weighed 245 grains, equal to 49 per cent. Glass grass green considerably transparent :— four grains of charcoal remained unacted upon.

No. 6.   Argillaceous iron ore .. .. .. 500 grs.
            Oxide of manganese roasted 100
            Charcoal ¼th 125

This mixture was accurately fused, and the result was a perfect metallic button weighing 246 grains, which is equal to 49$\frac{1}{10}$ per cent. Glass light green but clouded, transparent in thin pieces; not so bright as No. 5, but more transparent than No. 4. The same quantity of charcoal remained over in this as in the last experiment. From the three immediately preceding fusions with an excess of charcoal, it cannot be decidedly inferred that the increased produce beyond that of No. 1, 2, 3, was derived from the manganese; the reverse of this conclusion rather appears on the face of the experiments, seeing more metal was obtained without manganese than with it, in the proportion of 257 to 245 and 246. Lest any error should have taken place in the weighing of No. 4, which yielded 257 grains of iron, this experiment was twice repeated, and the results were 240 and 244, making an average of 245⅔.—Average of the fusions of No. 5 and 6 with manganese .. .. 245¼.

On the whole, it appears doubtful whether in these experiments there was any real alloy of manganese with iron; the solitary increase of weight in No. 3 and its peculiar fracture being the only circumstance in favour of this conjecture. On dividing and comparing the fractures of the metallic buttons, No. 4, 7, and 8 (the same experiment) were exactly alike. In these no manganese was used; the quality was white cast-iron, with a blueish glance, and a grain indicative of an approach to steel. No. 1 and 2 evidently belonged to the class of steels, so far as a commencement of grain was visible; but in respect to ductility or softness they were still closely allied to the hardest sorts of cast-iron: the former had experienced an excess of carbon, the latter had absorbed all that was presented to them in mixture. No. 5 and No. 6 were much alike, and similar to No. 3, in which de-oxidated manganese was used. The fractures were of a darker glance than 4, 7, 8, minutely but regularly granulated and crystallized. It appeared, therefore, that the

the addition of oxide of manganese to the amount of 1-5th the weight of the iron ore, alters the grain and fracture of cast-iron, where a sufficient quantity of charcoal is present to effect a perfect reduction of it, though it does not appear to add to, or impair to any observable amount, the quantity of metallic produce.

No. 9.   Argillaceous iron ore ..   ..   .. 500 grs.
     Oxide of manganese 150
     Charcoal as in the former experiments ᐧ 125

The result of this fusion was a crystallized radiated button weighing 247 grains, $49\frac{4}{10}$ per cent. Glass light blueish green, with considerable transparency. As part of it had escaped, the experiment was repeated, and an elegant prismatic crystallized metallic button again obtained, weighing 247 grains, which proved beyond any doubt the accuracy of the result.

No. 10.   Argillaceous iron ore   ..   .. 500 grs.
     Oxide of manganese 150
     Charcoal $\frac{1}{4}$th 125

Result from this fusion a crystallized button of metal weighing 240 grains, 48 per cent. Glass light-coloured flinty brown, with a good deal of transparency. Ten grains of metallic carburet were found unreduced; some globules of metal put on a smooth shining surface resembling iron highly charged with carbon.

No. 11.   Argillaceous iron ore   ..   .. 500 grs.
     Manganese roasted 250
     Charcoal $\frac{1}{4}$th, or   125

Result—A crystallized button and some carburetted globules weighing 253 grains, equal to $50\frac{6}{10}$ per cent. It was in this experiment, for the first time, noticed that some of the globules were not attracted by the magnet, and this peculiarity was attributed to a high alloy of metallic manganese with iron: the glass was of a lead-gray colour, transparent in thin fragments: nine grains of magnetic carburet of iron were found unreduced.

No. 12.   Argillaceous iron ore ..   ..   .. 500 grs.
     Manganese roasted 300
     Charcoal   125

The metallic button resulting from this fusion was accurately crystallized, and attracted by the magnet; there were, however, nearly twenty globules with smooth carburetted surfaces that were but slightly affected by the magnet. The whole produce weighed 260 grains, equal to 52 per cent. The glass was flinty clouded, but possessed a good deal of watery transparency.

No. 13.   Argillaceous iron ore   ..   .. 500 grs.
     Manganese roasted 400
     Charcoal   125

Although

Although the fusion of this mixture was most perfect, yet the diminished produce and colour of the glass indicated an excess of ores to charcoal. The metallic button weighed 243 grains, or $48\frac{6}{10}$ per cent. The quantity of resulting glass more than half filled the crucible; the colour dark grass green, transparent in thin fragments.

No. 14.   Argillaceous iron ore   ..   ..   250 grs.
             Manganese roasted   250
             Charcoal   ..   .. $62\frac{1}{2}$

A smooth skinn'd metallic button was the result of the fusion of this compound, weighing 112 grains, or $44\frac{8}{10}$ per cent. ; so that an increased demetallization of 4 per cent. had taken place, in consequence of increasing the proportion of manganese, beyond that of the last experiment.—The colour of the glass now obtained was dark green inclining to amber. From the result of these experiments it is evident that the alloy of manganese had reached its maximum in experiment 12, where a metallic return was obtained of   260

Average produce of three fusions of the iron
     ore *per se*   ..   .. $245\frac{1}{2}$
                                                    ⎯⎯⎯⎯

Increase from 300 grains of manganese, which ⎫ $14\frac{1}{2}$ grains.
     is not more than $4\frac{3}{10}$ per cent.   ..   .. ⎭

The metal of No. 12 will be composed as follows :
             Iron ..   .. 94·4 ⎫ 100 parts.
             Manganese ..   ..   .. 5·6 ⎭

No. 15. 200 grains of the black oxide of manganese used in these experiments were fused *per se*, and a perfect glass formed of a ruby wine colour, though not possessed of much transparency.

No. 16. 200 grains of deoxidated manganese were fused in a similar manner, and a glass similar to the last obtained.

No. 17.   Argillaceous iron ore   ..   ..   200 grs.
             Black oxide of manganese   200

This mixture melted into a perfect glass, metallic, and possessed of a lustre resembling that of a highly-polished razor-blade.

The three following experiments were made to ascertain the comparative demetallization of the oxide of manganese with a given quantity of charcoal, to exhibit the probable effects that might be produced on the burden of the blast-furnace in the event of ores of manganese being smelted along with our iron ores.

No. 18.   Argillaceous iron ore ..   ..   ..   500 grs.
             Charcoal $\frac{1}{10}$th   50

The result of this fusion was a smooth button of soft iron weighing 126 grains, equal to $25\frac{2}{10}$ per cent. Glass opaque, shining, and highly metallic.

No. 19.   Argillaceous iron ore   ..   .. 500 grs.
               Charcoal   50
               Oxide of manganese 200

Result of this fusion a metallic button weighing 55 grains, which is only equal to 11 per cent. being a diminution of 71 grains of iron, or upwards of 14 per cent., in consequence of the addition of 200 grains of manganese. Glass black, shining, metallic, opaque.

No. 20.   Argillaceous iron ore   ..   .. 500 grs.
               Charcoal   50
               Manganese roasted 200

The metallic button resulting from this fusion weighed 60 grains, or 12 per cent. The demetallization therefore occasioned by roasted manganese is less by 2 per cent. than when raw ore is used : this is of course occasioned by part of the oxygen of the ore being expelled from it in roasting, leaving so much less to act upon the charcoal of the experiment; the extent of the decarbonation in both being measured by the lack of metallic produce when the manganese was employed. In No. 19 a metallic deficiency was experienced of 71 grains, a quantity which required for its reduction 28 grains of charcoal : and in No. 20, a loss of 66 grains of iron, which would have, according to the result of No. 18, required 18 grains of charcoal.

From these facts it appears to me extremely probable that, should the ores of manganese ever be introduced into the smelting furnace, a considerable diminution of the carbonating properties of the coke will be felt; or, in other words, less burden will be carried, and more coke necessary to supply the waste occasioned by the combination of the oxygen of the manganese with the fuel. Experiment and practice alone will determine the comparative advantages and disadvantages that may result from so novel a proceeding, if ever introduced at our iron-works.

In most ores of manganese there is a portion of moisture and oxygen that may be expelled at a low heat; but the greatest quantity of the latter remains combined with the metal, and from which the last portions are not easily expelled even in the highest temperature of the essay furnace.

The essayist, not having the same means to guide him in fixing the perfection of his art on the development of a transparent glass (as is, or ought to be, the case in operating with ores of iron) is frequently left in the dark, and puzzled to determine whether he has obtained the whole metallic produce from the

ore

ore which is the subject of his experiments. Ores containing 30 per cent. and upwards of manganese, as in experiments 15 and 16, will fuse *per se* into glasses that will transmit light, and in thin fragments exhibit perfect transparency. This is not at all the case with ores of iron; a retention of 5 per cent. of iron will give an opaque bottle-green glass, and when it amounts to 10 per cent. the glass becomes black, shining, and metallic.

Recapitulation of the foregoing Experiments as to metallic Produce.

¼th the weight of the argillaceous ore of charcoal	No. 1	produced	$44\frac{3}{4}$	per cent.	No manganese
	2	do ..	$44\frac{3}{10}$	do	¼th manganese
	3	do ..	46	do	⅛th manganese

⅓th the weight of the argillaceous ore of charcoal	No. 4	produced	$51\frac{4}{10}$	per cent.	No manganese
	5	do. ..	49	do.	½th manganese
	6	do. ..	$49\frac{2}{10}$	do.	⅕th roasted do
	9	do. ..	$49\frac{3}{10}$	do.	⅓d manganese
	10	do. ..	48	do.	$\frac{3}{10}$ths do.
	11	do. ..	$50\frac{6}{10}$	do.	½ roasted do.
	12	do. ..	52	do.	⅗ths do.
	13	do. ..	$48\frac{6}{10}$	do.	⅘ths do.
	14	do. .:	$44\frac{8}{10}$	do	equal portions.

LXXIII. *On Flame.* By J. MURRAY, *Esq.*

To Mr. Tilloch.

SIR, — You have attached to my name the letters *M.D.* This distinction I have not assumed, nor do I covet it. I request you will rectify the error, lest my more humble efforts in the cause of truth and science might in some instances, by my imperfect details, detract from the *superior* merits of Dr. John Murray of Edinburgh*. By a typographical error *unhallowed* is printed " hallowed."

In some instances, by introducing into the flame of a candle a spatula of ivory with gunpowder, I have withdrawn it in a *moist* state; and from the circumstance that fulminating silver inserted into the cone of flame has not been exploded for some seconds,—I deem it probable that the interior is filled up with aqueous matter, which emanates from the interior surface of the flame, as well as exterior. This medium of aqueous particles

* Identity of name led us into the mistake alluded to, and must plead our excuse.—EDIT.

would

would account for the extinction of phosphorus, &c. when introduced.

In order to corroborate the solution I have proffered of the principle of security exhibited in the phænomena of Sir H. Davy's lamp, I may remark, that those persons who wear *veils* are sensible of a glow of heat exceeding the common temperature. Now in this instance the aqueous vapour which evolves in respiration fills up the open meshes of the veil, and from the interior surface the caloric is reflected back to the face. Either this is a correct solution, or it is accounted for by supposing that the calorific particles radiating in right lines, are interrupted in their rectilinear path by the fibres of the meshes, and being thus broken they are confined within the veil: surely the former speaks more the language of fact.

When the flame of a candle is viewed at a given distance through a good magnifying lens, it appears composed of a *congeries of fibres* wider at bottom, and approximating towards the summit, where they are bundled or tied together, being no doubt so many jets of aëriform inflammable matter expelled by heat.

Mr. Sym, in the last number of The Annals of Philosophy, says, that by pressing the apex of the flaming cone the temperature will be *decreased.* This may be naturally expected, and is easily explained. The top of the flame is as it were the focus where the fibres of flame are connected. By pressing upon the cone, this focus is destroyed. The fact of not being able to see objects through the upper part of the flame, while they can be readily discerned through the lower film, is proof positive that flame is transparent under certain forms—here it is attenuated.

The blue colour at the base of flame is worthy of particular notice. It is likely that the fibres of flame constituting this cone are so many capillary tubes into which the volatilized matter ascends by capillary attraction, and that oxygenous air circulates round the individual fibre. As they gradually approximate when they come towards the apex of the cone, there will be less and less space for the circulation of oxygen, and consequently the carbon will be more completely consumed toward the base than toward the middle summit:—this will well illustrate Sir H. Davy's experiments on flame, which I read with considerable interest.

If an oxygenous medium obtained in the interior of the cone of flame, I see no reason why it should not be a solid column. By igniting alcohol, &c. (without a wick) a hollow interior will be still found by pressing the apex.

No doubt in the phænomena of combustion and production

of

of water and carbonic acid gas, the theory of definite propor-
tions is beautifully preserved.

I feel now anxious to detail to you an experiment which may
serve to benefit the cause of humanity—a cause to which I pro-
fess myself warmly attached.   I came from the North on the
top of the Glasgow mail, and had " to bide the pelting of the
pitiless storm," the inside seats being pre-engaged from Carlisle.
On the road we encountered a fierce snow-storm.   The coach-
man and myself were nearly blinded by the unintermitting flakes,
and the poor animals the horses were as painfully situated, and
expressed as much. At the immediately succeeding stage I pro-
cured a piece of *black crape*, and attached it to the lining of the
hat, so that it hung vertically before the eye.   This I found
not only to prevent the snow from injuring the visual organ, by
warding it off; but at the same time, by cutting off the adven-
titious rays, I was enabled to obtain a more *distinct* and better
defined view than I else should have had.   This provision also
prevented the eye from being dazzled by the reflection of light
from the surface of snow.   Crape being an *uneven* surface, black
lace work with fine open meshes might serve better.

By mariners at sea, &c. this may be held to merit atten-
tion.

I am with high respect, sir,

Your obedient servant,

Surry Institution, Nov. 15, 1816.   J. Murray.

P.S.   I was much gratified to find the miners in a colliery near
Ayr, conducting their operations solely by the *light exhibited by
the fire-damp*—formerly so terrible an engine of destruction.

J. M.

LXXIV. *On the Circle.*

To Mr. Tilloch.

Sir, — A Correspondent of Dr. Thomson's (Annals of Phi-
losophy, June 1816, p. 468), has attempted to demonstrate that
no part of a circle is a straight line.   It may be supposed that
this proposition is so evidently correct, as to render demonstra-
tion unnecessary.   But however absurd the idea that curves are
composed of small right-lines may appear, it is, either directly
or indirectly, the foundation of most of the theorems relating
to curves.   A perfect circle can only be imaginary; there is no
such thing in nature, and art is incapable of producing one.

The

The following is the demonstration above referred to.—" In the arc AB of a circle take a small part, AC, which suppose to be a straight line. But if AC is a straight line, CB, which is equal to it, must be a straight line. For the same reason *n m*, which includes the point in which AC and CB are joined, is also a straight line; and therefore the whole arc AB is a straight line, which is evidently absurd. Therefore no part of a circle is a straight line.—Q. E. D."
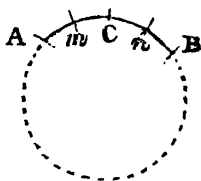
But let AC be a part of a regular polygon; then it is evident that all the conditions required by the above demonstration are fulfilled, and yet the figure is composed of straight lines.—Besides, when it is assumed that a circle is composed of small straight lines, the magnitude of each line is supposed to be infinitely small when compared with that of the circle of which it forms a part. Therefore the proposition does not appear to admit of a geometrical demonstration. But if such an assumption be necessary in the investigation of theorems, there can be no good reason for rejecting it, unless it can be proved that no part of a circle can be a straight line.
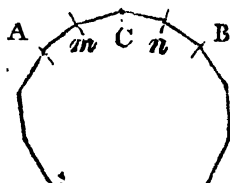
June 5, 1816. T. T.

LXXV. *On the Origin of the Atomic Theory.* By WILLIAM HIGGINS.

To Mr. Tilloch.

SIR, — YOU will oblige me and every lover of justice by inserting in your impartial Magazine the following remarks on an article relating to the *Atomic Theory*, published in the first volume, part second, of the fourth and fifth editions of the *Encyclopædia Britannica*, which article was written by Dr. Thomson.

The Doctor commences by stating, that the most eminent of the Greek philosophers supposed the ultimate elements of matter to consist of atoms or particles incapable of further division or diminution; and that this doctrine was adopted by Sir Isaac Newton, and by many celebrated philosophers since his time. This is perfectly correct; but they had not the most distant idea of its application to chemistry in the sense in which I had taken it up.

The Doctor proceeds by giving a sketch of the progress of chemistry

chemistry as it passed through the hands of different philoso-
phers. He particularizes Cullen, Black, Cavendish, and Priestley,
whom we must ever revere with grateful feelings for their suc-
cessful labours in the field of science.

To each of those philosophers, except Dr. Cullen, who was
before my time, I had the pleasure of presenting a copy of my
"Comparative View of the Antiphlogistic and Phlogistic Theo-
ries," printed in the year 1788, and published early in the year
1789.

In writing this work, happily for chemistry, arose during my
investigation of the two antagonist theories the Atomic System,
or more properly speaking the doctrine of Definite Proportions,
in which the elementary particles of matter are capable of uniting
so as to form atoms and molecules*.

My atomic theory of chemistry is so mathematically correct,
that all visionary hypotheses fell prostrate before it, and it was
from it *alone* that the phlogistic doctrine received its fatal blow.

When I published the above work, I was the only person in
Great Britain that adopted the antiphlogistic doctrine : and the
attention of the philosophical world was so anxiously engaged
in the controversy itself, that the novel mode of investigation
which I made use of, was at the time in a great measure over-
looked : and indeed I was not much surprised at it, for the
science was not at that time sufficiently ripe for so unusual a
style of reasoning.   However, some of the Reviewers of the day
took some notice of it, as the following extract, taken from the
fourth volume of the Analytical Review, page 178, will show:

" This is the first original publication (my Comparative View)
which has appeared in the English language in defence of the
antiphlogistic system of chemistry, which is here very ably main-
tained by Mr. Higgins."   It then proceeds with the division of
the work, and the questions discussed in it. " In the discussion
of this subject Mr. Higgins shows a degree of acuteness in argu-
mentation, and an intimate acquaintance with the present state
of chemistry, which prove him to be eminently qualified for the
task he has undertaken.   In addition to those requisites, we
find that he has actually repeated most of the leading experi-
ments ; which valuable circumstance places his book in a much
higher rank than that of a mere collection of facts and deduc-

* The terms ultimate, particle, atom, and molecule, are indiscriminately
used by Thomson and Dalton.   An ultimate particle is the last division of
elementary matter—an atom is a compound of two particles in every pro-
portion—and a molecule is the compound of two atoms according to the
strict nomenclature of my doctrine. Those distinctions will prevent con-
fusion ; they will be found to accord with the language of definite propor-
tions, and the internal structure of compounds,

tions. His style and arrangement is strong and perspicuous, al-
though we here and there meet with inaccuracies which denote
he is not yet familiarized to the art of composition. The che-
mical reader will readily form an idea of the facts contained in
the sections whose titles we have enumerated. They consist in
a great measure of those which we have already mentioned in an
account of the French edition of Kirwan on Phlogiston*: the
statements however are very different, and the elucidations both
of theory and matter of fact are in many instances original and
striking. We do not therefore hesitate to recommend this per-
formance of Mr. Higgins, as a work well deserving the attention
of chemists: but as it would lead us too far into chemical dis-
quisition to follow him step by step in the enumeration of facts
and display of arguments which cannot be abridged, we shall
conclude this article by another quotation; in which as much
appears to be said and done to establish the composition of
water against the late experiments of Dr. Priestley as the pre-
sent state of the subject appears capable of."

In about twelve months after my book appeared, Dr. Priestley
was the only phlogistian in England, and he retained his old
tenets to the last moment of his life. I do not recollect the
exact time Dr. Black recanted, it was after Kirwan. Mr. Kir-
wan, the formidable champion of the phlogistic doctrine, re-
nounced it as soon as he read my book, and declared in the
presence of many philosophical gentlemen now living in Dublin,
that it was that work alone induced him to change his opinion,
and that nothing the French philosophers brought forward had
any influence on him; this appears from his notes in answer to
the French at the end of the English translation.

Dr. Thomson tells us in the fourth volume of his Annals,
p. 54, that it was the answer of the French chemists to Mr.
Kirwan's Essay on Phlogiston that decided this memorable con-
troversy. Nothing can be so incorrect or so unjust as this as-
sertion: for the answer was published before I wrote; and from
the foregoing statement, which is a true one, it is evident that
it produced little or no effect; and it appears by the extract
from the Analytical Review that my demonstrations were consi-
dered as original at the time I had written†. These were my
principal motives for introducing the foregoing subjects.

But to return to the outlines of the Doctor's history. He at-
tributes, and very justly, the first rudiments of analytical che-

* This edition contains the answer of the French chemists to that work;
and that is one of my principal reasons for inserting it here, as will imme-
diately appear.
† It alludes to the Atomic System.

mistry

mistry to the labours of Margraaf, Bergman, and Scheele. Kirwan, Bergman, and Wenzel distinguished themselves by the analyses of the salts. They ascertained that salts and all compound bodies are united constantly in the same proportions of their constituents.

The foregoing philosophers had written long before me; and I challenge Dr. Thomson to produce a single page from their respective works that relates to the atomic theory, or, in other words, to the definite proportions in which elementary particles unite so as to form atoms and molecules.

From the foregoing philosophers the Doctor passes to Richter, who analysed saline bodies with still greater accuracy than his predecessors. He ascertained the quantity of the earths and alkalies necessary to saturate 100 parts of different acids. As the labours of this chemist do not relate to the atomic theory, I consider it unnecessary to attend to them minutely.

Had Dr. Thomson been a faithful and unprejudiced historian, he would have had the candour to mention my *Comparative View*, and the discovery of the atomic theory, before he brought forward the labours of Richter, which were subsequent to mine.

Next in the order of this curious history, Proust (no doubt a chemist of considerable merit) is introduced. From the great attention which he paid to metallic oxides, he was able to prove that every metal is capable of forming a certain determinate number of oxides, and no more. " Thus, zinc unites but to one dose of oxygen; consequently there is but one oxide of that metal: iron, arsenic and antimony form two each: tin forms three."

In my *Comparative View*, written many years before the work of Proust, it will be found that I considered metals in general to be capable of uniting to different doses of oxygen, and that the force of union was in the inverse ratio of the number of doses which they took in*. I mention these circumstances merely to show that I developed principles only, for I had not attended to the different doses of oxygen to which the different metals were capable of uniting. These circumstances ought to have been mentioned: but this would frustrate the Doctor's purpose, that of bringing Dalton in as an original discoverer of the atomic theory.

" Such was the state of the subject," continues the Doctor, " when Mr. Dalton turned his attention to the combinations of bodies with each other, about the year 1804." Mr. Dalton's first volume of the Atomic Theory made its appearance in 1808.

* I refer to my Comparative View, or to my Atomic Theory, on this subject.

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The Doctor acknowledges with *wonderful* candour, that it was known at this period that hydrogen unites only in one proportion with oxygen; that carbon, sulphur, and phosphorus unite in two proportions; and so he goes on enumerating other combinations long known before this period.

It would be needless to follow the Doctor through all his details, most of them being well known; and many misrepresentations are brought forward, in order to prepare the way for his ingenious friend to take possession of the Atomic System.

The proportions in which inflammable substances and oxygen are found to unite, such as 1 and 1, or 1 and 2, &c. by weight, " led Mr. Dalton to the *lucky idea* that the atoms of bodies unite together; that the atom of each body has a determinate weight, and that this weight regulates the proportion in which bodies combine. Let us suppose, for example, that water is formed by the union of one atom of oxygen with one atom of hydrogen; it follows, as the oxygen in water is eight times that of the hydrogen, that the weight of the atom of oxygen is to that of an atom of hydrogen as 8 to 1. So that if we represent the weight of an atom of hydrogen by 1, that of an atom of oxygen will be 8." The Doctor adduces many more examples of this kind, which first appeared in my *Comparative View,* as shall be presently shown.

"But Mr. Dalton," continues the Doctor, " not satisfied with this simple and luminous explanation, which threw a new and strong light around chemical combinations, which afforded the means of correcting and checking chemical experiments hitherto conducted without any guide, and promised in time to introduce mathematical precision and mathematical reasoning into a science which hitherto has been able only to boast of analogical and probable conclusions —— contrived a set of symbols to represent the different elements, and make the nature of the combinations which they form obvious to the eye of the most careless reader." An engraved specimen of those symbols is given, so far as they relate to the ultimate particles of hydrogen, azote, carbon, sulphur, phosphorus, and oxygen. The symbols representing the inflammable particles or bases are united to those representing particles of oxygen in the proportion in which they are capable individually of combining with that element, that is, either 1 and 1, 1 and 2, 1 and 3, or 4, &c.

These diagrams, if diagrams I can call them, are much more correct than Dalton's[*]. They correspond with the proportions of elementary particles represented by diagrams so as to constitute the same compound atoms in my Comparative View, ex-

[*] See his work, or my Atomic Theory, where the original symbols are given.

cept

cept in those of sulphurous and sulphuric acids, which are re-presented in erroneous proportions, as I had shown in the above work, and lately confirmed by additional experiments in my *Atomic Theory*. There is another error in this table of dia-grams or symbols which I cannot pass over; that is, the leaving out an intermediate state of the combination of azote and oxy-gen between the nitrous and nitric acids, which I represented as containing one of azote and four of oxygen. Thus the Doctor takes a stride from 1 and 3 of those elements to 1 and 5. He falls into the same error respecting some metallic oxides. But probably those gentlemen had a motive to differ from me, *right or wrong.*

"It would be easy," continues the Doctor, "to multiply these symbols much further; but the preceding specimen is sufficient, we conceive, to make the use of them understood, and even to make Mr. Dalton's doctrine more simple to those who are still strangers to it."

I agree with the Doctor; and I will go still further, by saying that they develop the whole essence and spirit of the Atomic System.

I will now proceed so far, in as concise a manner as possible, on what I advanced on the very same subject in the year 1788, twenty years before Dalton published his first volume.

The Union of Oxygen and Hydrogen.

1. Two volumes of hydrogen unite to one volume of oxygen, and in no other proportion whatever.

2. The two volumes of hydrogen contain the same number of ultimate divisions or particles that is contained in the one of oxygen, notwithstanding the difference of their specific gravities; and this difference depends on the size of their respective par-ticles.

3. Hydrogen and oxygen unite chemically,—a single ultimate particle of the one to a single ultimate particle of the other,—to constitute an atom of water. A diagram representing this com-bination, with numbers representing the energy or force of union of its elements, was given.

Have not the foregoing facts clearly shown the weight of an atom of water?

Sulphur and Oxygen.

1. An ultimate particle of sulphur unites to a single ultimate particle of oxygen, and the compound constitutes an atom of sulphurous acid gas; and as oxygen gas suffers no material di-minution by the union, and as pure sulphurous acid gas is but twice the weight of oxygen, it was presumed that the ultimate

particle

particle of sulphur was of the same weight with that of the oxygen, and consequently that the size of the calorific atmosphere of the acid atom was as large as that of the ultimate particle of oxygen before the union.

A diagram representing this combination, with numbers expressive of the force of union comparatively with that of the constituents of water, is given.

2. An ultimate particle of sulphur is capable of uniting to two ultimate particles of oxygen, and the compound is an atom of sulphuric acid. These are the definite proportions in which those two elements can unite.

3. The two portions of oxygen are united with less energy in the sulphuric acid than the one proportion in sulphurous acid. A diagram with numbers representing this difference was introduced.

Metals and Oxygen.

The ultimate particles of some metals,—I instanced iron,— attract oxygen with greater force than those of sulphur or hydrogen do. This superiority of force was also expressed by numbers.

The relative forces with which the foregoing inflammable bases attracted oxygen, and the diminution of this force by double doses of oxygen, enabled me to calculate the changes and different phænomena which are produced by metallic substances when exposed to the action of sulphuric acid, dilute sulphuric acid, and sulphurous acid. It enabled me not only to point out the absurdity of the phlogistians, but also the errors of the antiphlogistians themselves. What I here assert will be found verified in my *Comparative View*, or in my *Atomic Theory*.

Azote and Oxygen.

1. One ultimate particle of azote and one of oxygen chemically united constitute an atom of the gaseous oxide of azote.

2 When united to two of oxygen an atom of nitrous gas is formed.

3. When the particle of azote is united to three particles of oxygen an atom of the red nitrous acid is formed.

4. When the azote unites to a fourth particle of oxygen an atom of the straw-coloured nitrous acid is the result.

5. When united to a fifth particle of oxygen an atom of nitric acid is formed. These are the definite proportions of oxygen and azote.

All the foregoing distinct compounds of azote and oxygen, and the different forces with which they retain their oxygen, are represented

presented by diagrams; the energy of union gradually and regularly diminishes from the *minimum* to the *maximum* state of oxygenation. Each of the atoms of even the nitrous and nitric acids in the liquid mass are distinct from one another, as being surrounded with their respective atmospheres of caloric.

When the foregoing compounds produced by the union of azote and oxygen are mixed, a partial decomposition takes place, and the oxygen is divided between them ; but that which contains 1 and 2 can take none from 1 and 3, but it will take from 1 and 4, and 1 and 5, or 1 and 3 can take from 1 and 5, but not from 1 and 4.

Nothing could be more easy than to deduce the weight of these atoms from the relative weights of their constituent gases.

This arrangement and calculation of the force of union of the constituent elements of the nitrous gas, of nitrous acid and nitric acid, enabled me at that remote period to demonstrate with mathematical accuracy all the chemical effects produced on those acids by inflammable bodies, which were not understood before.

Nitrous Acid and Potash.

A single atom of nitrous or nitric acid unites to a single atom of potash, and this molecule is surrounded with an atmosphere of caloric which renders each molecule in the saline mass independent of each other. This holds good with respect to all other saline substances.

Metals and Oxygen.

The ultimate particles of a metal unite with different doses of oxygen : the first dose is retained with greater force than the second ; and this last with greater force than a third dose. Tin was adduced as an example in my Comparative View.

Metals and Acids.

Metals, or rather their oxides, unite with acids atom and atom so as to form molecules, and the force of union depends on the kind of metal. I have shown that the metals first unite to oxygen, and that their ultimate particles are supplied with it at the expense of the water, or of a portion of the acid itself. To illustrate this point diagrams were produced.

The precipitation of some metals in their metallic state from their solution in acids by other metals had been explained upon strict mathematical principles, by means of diagrams, and each ultimate particle was allowed its full force and effect in the operation, agreeably to the laws of the atomic doctrine. When one metal precipitates another metal from its solution in a *semi* state

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of oxidation, it is because the precipitant has less capacity for oxygen than the precipitated: this was: also explained on the atomic principle.

The foregoing short sketch comprehends nearly the whole of the atomic theory, or of definite proportions. Its application in chemical researches is a secondary business. No theory can be confined to the labours of its author. The great excellence of a doctrine depends on its capability of being universally applied throughout the whole range of chemical science,—a task too great for any individual.

[To be continued.]

LXXVI. *Observations of the late Solar Eclipse. By* S. GROOMBRIDGE, *Esq.*

To Mr. Tilloch.

SIR, — I REQUEST you will be pleased to insert the following observations of the late solar eclipse, in the Philosophical Magazine; which, from its extensive circulation, will enable those who have observed the same phases of the eclipse to compare the different results.

19th of November A.M., mean time.

8ʰ 4′ 29″ immersion of moon's disc 6′ 9″ from vertex.		
8 10 58 vertical distance of cusps 10′ 32″.		
8 15 2 west cusp from upper limb of sun ..	12′	50″
8 21 46	15	21
8 27 48 .. .. lower limb of sun ..	15	28
8 33 48	14	39
8 46 2	14	21
8 49 42 south limb of moon from south limb of sun	10	37
8 52 34	9	55
8 56 18 occultation of the largest spot.		
8 58 26 .. .. second large spot.		
8 59 28 .. .. third large spot.		
9 2 6 south limb of moon from south limb of sun	8′	1″
9 7 4 east cusp from south limb of sun ..	14	14
9 9 8 south limb of moon from south limb of sun	6	25
9 11 56 east cusp from south limb of sun ..	9	18
9 15 54	6	25
9 17 48	5	16
9 20 20	4	8
9 22 14 south limb of moon from south limb of sun	3	40
Becomes cloudy.		
10 12 50 vertical distance of cusps 17′ 7″.		
10 19 18 emersion of moon's disc 12′ 19″ from south limb.		

A a 2 These

These observations were made with an achromatic telescope of 46 inches, on an equatorial stand ; and the measures taken with a micrometer on the vertical diameter of the sun, on the plane of the equator. These measures being the versed sines of the sun's semidiameter 16′ 13″·5, will give the several angles from the vertex of the sun.

Versed Sines.		Angles.	
6′ 9″	·3788	51° 36′	immersion.
12 50	·7915	77 58	west cusp.
15 21	·9463	86 55	.. ..
15 28	·9532	92 41	.. ..
14 39	·9033	95 33	.. ..
14 21	·8844	96 38	.. ..
14 14	·8779	97 1	east cusp.
9 16	·5711	115 24	.. ..
6 25	·3955	127 12	.. ..
5 16	·3248	132 28	.. ..
4 8	·2546	138 12	.. ..
12 19	·7595	103 55	emersion.

It was noticed that an error has been published in the Nautical Almanac, of the part of the sun's disc, where the first impression would be made, being 59″ from the vertex. The primary construction of a solar eclipse being made on the ecliptic, I am of opinion, that it is computed for the vertex of that plane ; which should have been so explained. When vertex is mentioned without reference to the plane, we generally suppose it to be the visual vertex on the plane of the horizon. The immersion happened about half a minute later than computed, and as the moon was decreasing in north latitude the impression would be further from the vertex : on the plane of the ecliptic the angle was 64° 55′, on the plane of the equator 51° 36′, and on the plane of the horizon 20° 37′.

The undulating stream of light round the disc of the moon should seem to indicate an atmosphere on the surface of that body; and also the disc did not appear to be perfect, but serrated; both which circumstances were rendered more observable from the intense light of the sun. *

This eclipse has been total in the northern parts of Russia, on and near the meridian; since the moon's diameter exceeded that of the sun.

<div style="text-align:center">I am, sir,</div>

<div style="text-align:center">Your obedient servant,</div>

Blackheath, Nov. 22, 1816.   S. Groombridge.

LXXVII. *Inquiries into the Laws of Dilatation of Solids, Liquids, and elastic Fluids, and on the exact Measurement of Temperatures. By Messrs.* DULONG *and* PETIT. *Read to the Institute* 29*th of May* 1816*.

WHEN we endeavour to scrutinize most of the questions relative to the theory of heat, our progress is soon arrested by a difficulty which reappears at every step under different forms. What are we to understand by the word temperature? and what is the relation which exists between the indications of the thermometer and the quantities of heat added or subtracted, in order to produce determinate variations in the temperature? Here are two questions which must be resolved ere we can find, for example, the true laws of cooling: and it was, in fact, in order to solve them, that we embarked in the inquiries, a part of which we now submit to the judgement of the Class.

The construction of all the instruments destined to measure temperatures rests on the property possessed by bodies of changing their volume by the action of heat; but these instruments, in order to be rigorously exact, ought to satisfy two conditions equally indispensable. The first is, to be capable 'of comparison with each other, *i. e.* to agree always in their indications: the second is, to be comparable with themselves, *i. e.* to have a course which shall be in a known ratio with the variations of temperature.

The first of these conditions may be now obtained with great exactitude. We know well all the causes which can influence the justice of the indications of these instruments, and by employing the proper precautions we may attain a precision sufficient for all observations.

It is much more difficult to satisfy the second condition, which consists in establishing a graduation in which the equal parts of the scale answer to equal variations of temperature. In fact, in order to fulfil it completely, it is not sufficient to determine the relation of dilatations undergone by the thermometrical substance which we employ, to the quantities of heat which we communicate to it: we must also ascertain that the capacity of this substance for heat does not change, or at least keep an account of this change if it takes place. The extreme difficulty of determining with precision how the specific heat of a body varies, particularly at high temperatures, may be regarded as one of the greatest obstacles to the direct solution of this question.

Experimentalists, however, have made some attempts to attain it. De Luc, who was the first, and is perhaps the only one

who

who took up the subject, supposed that the capacity of water does not vary between 0 and 100°; so that, by mixing two equal masses of this fluid at different temperatures, he took for that of the mixture the mean of the temperatures of the separated masses.

Mr. Dalton thinks that it is not equal masses which must be mixed, but rather equal volumes; because he supposes that the capacity of the same mass increases with the temperature in proportion to the volume which it acquires; or, in other words, he supposes that the capacity of bodies referred to their volume will remain constant. It is very easy to prove that the specific heat of the gases, and even of the liquids, undergoes variations in the ratio indicated by Mr. Dalton; but we do not find in the work of this eminent philosopher any experiment in support of the law which he seeks to establish.

We see that the researches hitherto set on foot on the true gradations of a thermometer are reduced to very little, and we may even add that no person has undertaken direct experiments to ascertain them beyond 100°.

After having compared with each other the different experimental methods which may be employed in order to attain the solution of this important question, we have thought that the determination of the quantities of heat, particularly at high temperatures, was not susceptible of sufficient precision. We thought it preferable to compare, in the first place, with the mercurial thermometer the progress of the dilatation of bodies the most homogeneous, and of such a nature that the causes which visibly affect the uniformity of the dilatation should have no influence over them. These bodies ought evidently to be taken from among the gases, or from among the solids endowed with a great infusibility. The experiments of M. Gay Lussac having taught us that all the gases are dilated exactly in the same way when they are placed in the same circumstances, it is natural to conclude that the dilatability of one and the same gas ought to be constant, and that consequently, at increments equal in volume or in elastic force, they ought to answer to equal increments of temperature.

It was by setting out from this principle that M. Gay Lussac ascertained that the march of the mercurial thermometer was regular between the freezing and boiling points.

Experimentalists are pretty generally agreed in regarding the dilatation of solids which are very difficult to melt as being uniform. The experiments of Messrs. Laplace and Lavoisier on the dilatation of most of the metals between 0 and 100°. come in aid of this opinion.

Now, if bodies so different as the metals and the gases followed
<div align="right">lowed</div>

lowed the same course in their dilatations, it will become extreme-
ly probable that these dilatations will indicate the true tempera-
tures : this is what might besides be verified by the correspond-
ing quantities of heat. By following this course, we shall have
the advantage of bringing into the determination of the true
thermometrical scale, all the precision which we may now give
in the measurement of dilatations; and these measurements them-
selves, independent of the important consequences which we
might deduce from them relative to the theory of heat, will pre-
sent data which will be useful under several circumstances. Such
are the considerations and the motives which determined us to
commence our labours by the comparison of the dilatations of
the gases and the solids with the mercurial thermometer in high
temperatures.

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Comparison of the Dilatation of the Gases and of the Progress of the mercurial Thermometer.

The apparatus which we used was composèd of a rectangular
tub of red copper, seven decimetres long, one decimetre broad,
and a decimetre deep. This tub has on one of its lateral faces
two openings, one of which serves to introduce, in a horizontal
situation, a mercurial thermometer, and the other holds the open
extremity of a tube which is placed horizontally at the same
height with the thermometer. This tube is perfectly dry, and
contains air which is dry also.

The tub rests on a fiftnace constructed in such a manner as to
heat all parts equally. It is filled with a fixed oil, which
may, as we know, support a temperature of more than 300 de-
grees without boiling. The tube which contains the air is ter-
minated on the side of the aperture by a short tube of a very
small diameter, which partly issues from the tub. The quan-
tity of air contained in the exterior portion of this tube, and
which does not participate in the heating of the rest, is not
worth noticing. We ascertained that it never exceeded a two-
thousandth part of the total mass ; and besides, we had the pre-
caution to heat it during every experiment, in order to reduce
the error which might result from it.

The tub is covered by a lid pierced with several holes: some
are traversed by thermometers, which serve to indicate if the dif-
ferent parts of the liquid mass are at the same temperature; the
others have stalks or sticks terminated by vertical plates of cop-
per, which we can turn ; we thereby produce in the liquid a
brisk agitation, the object of which is to establish the uniformity
of temperature.

The following is the course pursued by us in every experi-

ment. We heated, in the first place, the tub to a temperature
little inferior to that which we wished to obtain, and we after-
wards closed all the apertures of the furnace. The equilibrium
of heat tending then to be established throughout the whole
heated mass, the temperature of the oil still rose some degrees,
and soon attained its maximum, where it became, some time
stationary, and consequently easy to measure with precision.
It was then indicated by the horizontal thermometer, which was
sunk sufficiently in the oil, in order that the whole column of
mercury should be inserted in it: at the same instant we closed
by means of the blowpipe the fine point of the external part of
the air-tube, and we noted the barometrical height. This being
done, we withdrew the tube and carried it into a separate cham-
ber, the temperature of which was nearly invariable : we placed
it vertically, and in such a way that the point entered a mer-
curial bath perfectly dry. We broke this point in the mercury,
and this liquid ascended until the equilibrium was established
with the external pressure : we left it in this situation a suffi-
cient time to give it precisely the temperature of the room,
which we knew with great precision by means of a very sensible
thermometer suspended beside the tube. When the equilibrium
of temperature was perfectly established, we measured, by
means of a vertical scale fitted with an index, the height of the
column raised in the tube. We observed at the same time the
barometrical height, and the difference of these heights made us
acquainted with the elasticity of the cold air. We then with-
drew the tube, by taking all necessary precaution to retain the
mercury of which the column that had been raised was com-
posed. The tube and the mercury which it contained were
weighed: we afterwards weighed this same tube successively
empty and entirely full of mercury; by subtracting from the
result of this last weight those of the first two, we had the weight
of two equal volumes of mercury, the one with the volume of
hot air and the other with the volume of cold air ; and from
these weights we inferred the volumes themselves, which we
afterwards brought to what they would have been under the
same pressure, since we knew the elasticity of the cold air which
had been measured as we have indicated, and that of the warm
air which was equal to the pressure of the external air observed
at the instant of closing the tube.

In order to judge more easily of the degree of confidence which
the results deserve to which we had been led, it will not be un-
availing to give some details relative to the precautions which we
took in this experiment. One of the greatest obstacles which
we met with in this description of inquiries is owing to the dif-
ficulty of establishing a perfect uniformity of temperature in a
<div align="right">great</div>

great liquid mass 200 or 300 degrees warmer than the surrounding air. We attain this rigorously when the temperature at which we operate is, for example, that of the ebullition of the liquid which we employ: then this temperature is necessarily fixed; but in every other case the progress more or less rapid of the heating or cooling of the different points of the mass, opposes the necessary uniformity from taking place We are of opinion, however, that the arrangement of our apparatus remedies in a great measure this kind of inconvenience, and this is owing, on the one hand, to the copper tub being sunk in the furnace, and composing with it a considerable mass, which is cooled slowly, particularly when it is near its maximum of temperature; and in the second place, the liquid being continually agitated, the heat ought to spread through it more uniformly. In short, to remove all doubts, we inserted into this tub two thermometers situated horizontally at the same height; we had raised the temperature in the same way as in our ordinary experiments, and we ascertained that by shaking the liquid the thermometers never differed but by a few tenths of a degree.

Besides, even supposing that the different particles of the liquid layer which surround the air-tube were not exactly at the same temperature, the error will not be so great as at first believed; for, in consequence of the arrangement of the apparatus, the bulb of the thermometer nearly answers the middle of the length of the tube, and consequently this instrument ought in all cases to indicate a temperature not far from the mean of the different parts of the tube. We ought to recollect also, from what has been said, the necessity which there is, in order to know the true indications of the thermometer, to sink it sufficiently in the liquid, that the whole column of mercury should enter it. This precaution, which appears to be unnecessary in low temperatures, ought not to be omitted in high temperatures; for then the column of mercury contained in the tube may undergo an increment of very sensible length. Thus we have remarked that, at the temperature of 300° for instance, there was frequently more than 15° of difference between the indications of one and the same thermometer, whether the whole of the mercury was in the liquid, or the bulb only entered it. We might indeed, according to our knowledge of the dilatation of the mercury, estimate the error which we commit by plunging only part of the thermometer into it; but the correction to make in this case, bearing on considerable numbers, will occasion serious errors, because we never know exactly the mean temperature of the mercury contained in the tube. It appeared to us to be preferable in all cases to place the thermometers horizontally. In order to give a more precise idea of the various operations of which each of our experiments is composed, we

we shall subjoin a complete series, with all the indications neces-
sary for calculating them.

This series does not comprise any observations for temperatures
approximating 100°, although we have repeated several times the
experiment of the dilatation of the air in boiling water. We did
not propose by any means to verify thereby a determination on
which no doubts can be entertained; but the coincidence of our
result with that of M. Gay Lussac has been for us the best proof
of the rigorous exactitude of the process which we made use of.

Temperature of cold air.	Temperature of warm air.	Elasticity of cold air.	Elasticity of warm air.	VOLUME in centimetre cubes,		Temperature deduced from the dilatation of the air.
				of cold air.	of warm air.	
17°,06	156°,85	0,6186	0,7653	63,526	76,438	155°,7
16°,74	197°,53	0,5771	0,7561	34,8573	43,287	194°,64
18°,25	249°,43	0,55695	0,7594	53,225	69,862	243°,25
18°,24	318°,11	0,52525	0,7603	66,1728	92,2875	309°,7

We have made several similar series of experiments and nearly at
the same temperatures. By a very simple interpolation and by
taking mediums, we have formed the following table, which indi-
cates the correspondence of degrees marked by the mercurial
thermometer, and those which are deduced from the dilatation of
the air.

Temperatures indicated by the mercurial thermometer.	Temperatures deduced from the dilatation of the air.	Differences.
100°	100°	0°
150°	148° 70	1° 30
200°	197° 05	2° 95
250°	244° 17	5° 83
300°	291° 77	8° 23

Although the experiments which we have given present a re-
markable coincidence, we thought it right to attempt the attain-
ment of the same results by a different route.

In these new experiments we made use of an air-tube of a
much greater capacity than in the first, and we placed it in the
same way, only the very narrow tube which was soldered to it
was curved at its issuing from the tub, and prolonged vertically
for the length of almost five decimetres : it was heated by taking
all

all the precautions which we have mentioned; and when we had attained the stationary temperature and had noted the baro-metrical height, we conveyed under the inferior extremity of the vertical tube a capsule full of very dry mercury : the tube was al-lowed to cool until the oil had resumed nearly the temperature of the air. During the continuance of this cooling, the mercury ascends into the vertical tube, and does not stop until the air con-tained in the tube is completely cooled. The elastic force of this air is then equal to the external pressure of the atmosphere di-minished by the height of the column raised up : that of the warm air was equal to the barometrical height observed at the instant when the temperature was stationary? We might therefore con-clude, by means of the law of Mariotte, what would have been the dilatation of the air if it could have been dilated. In order to render this process completely exact, we ought to have kept an account of the capillary depression which the mercury undergoes in the straight tube into which it had risen. This depression had been measured beforehand, and care had been taken to make choice of a tube of such a calibre as not to admit of its varying sensibly.

In the second place, the volume of air did not remain exactly constant; the portion comprised in the vertical tube re-entered in part into the great tube, in proportion as the column of mercury rose, and this portion of air did not sensibly change its temperature. It became necessary to calculate the influence of these two causes, and make our observations undergo the correc-tion flowing from it. This correction depended on the relation of the capacity of the large tube with that of the small; the calcu-lation which gives it is besides too simple to render it necessary to indicate it. We shall now give the results of one of the series of experiments made by this new process: all of them agree with the observations made by the first means, and they have entered into the determination of the mean measurements formerly re-lated.

	Millimetres.	Capillary Correction.
Length of the great tube ..	0 62	millimetres
Length of the small tube ..	0 57	4·5.

Elasticities of the warm air corrected from the capillary depression.	Temperatures corresponding observed on the mercurial thermometer.	Temperatures calculated ac-cording to the variation of the elastic force of the air,
0, 48,60	91°,417	91°,417
0,579,45	163°,21	160°,27
0,706,85	263°,8	254°,14
0,765,75	309°,88	297°,54

The results which we have made known inform us that the dilatation of the mercury in the thermometer follows a more rapid course than that of the air; so that, if we regard the latter as necessary to serve for the exact measurement to the temperatures, we ought to conclude that the indications of the mercurial thermometer are too high in the temperatures superior to that of boiling water, and the numbers which we have given might serve to make these indications undergo the proper corrections. Those numbers increase besides in a sufficiently regular manner, in order that we may without sensible error determine the correction relative to the temperatures intermediate to those which are comprehended in the table.

This conclusion destroys a doubt which was raised with respect to the law of dilatation of the gases. This law had not been announced in the same way by Messrs. Gay Lüssac and Dalton, whose experiments on the subject now before us appeared at the same period. The experiments of M. Gay Lussac proved that the dilatation of the gases referred to the mercurial thermometer is for each degree a constant fraction of the volume at a determinate temperature. Mr. Dalton, on the contrary, supposed that the increase of the volume is for each equal variation of temperature, a constant portion of the volume at the foregoing temperature. In truth, Mr. Dalton does not appear to have made direct experiments on this head: the only arguments which he advances in favour of his hypothesis are reduced to the extreme simplicity under which laws in appearance very complex will then be presented, such as the law of the cooling of bodies, and that of the variation of the elastic force of steam.

We ascertained, however, that the first of those laws will by no means assume a character so simple as he pretends, even admitting his hypothesis to be correct.

To conclude: It is not by considerations of this kind that we can establish laws which observation alone ought to furnish. The experiments which we have made at high temperatures completely destroy the hypothesis of the English experimentalist: for, so far from these experiments pointing out any thing perfectly positive as to the measurement of temperatures, it is at least very probable that the march of the mercurial thermometer ought to be more rapid than that of the temperatures, since in all the other liquids the dilatability increases in proportion as they are heated; whereas in the hypothesis which we are attacking, we shall find, on the contrary, that the dilatability of the mercury decreases rapidly in proportion as it is heated; a result completely opposite to the very principles on which Mr. Dalton had founded his theory of the measurement of temperatures.   [To be continued.]

LXXVIII. *Notices respecting New Books.*

T HE Second Part of the Philosophical Transactions of the Royal Society of London, for 1816, has just appeared, and the following are its contents:

11. An Essay towards the Calculus of Functions. Part ii. By C. Babbage, Esq. Communicated by W. H. Wollaston, M.D. Sec. R.S.—12. Experiments and Observations to prove that the beneficial Effects of many Medicines are produced through the Medium of the circulating Blood, more particularly that of the Colchicum autumnale upon the Gout. By Sir Everard Home, Bart. V.P.R.S. Communicated by the Society for improving animal Chemistry.—13. An Appendix to a Paper on the Effects of the Colchicum autumnale on Gout. By Sir Everard Home, Bart. V.P.R.S.—14. On the cutting Diamond. By W. H. Wollaston, M.D. Sec. R.S.—15. An Account of the Discovery of a Mass of Native Iron in Brasil. By A. F. Mornay, Esq. in a Letter to W. H. Wollaston, M.D. Sec. R.S.— 16. Observations and Experiments on the Mass of Native Iron found in Brasil. By W. H. Wollaston, M.D. Sec. R.S.— 17. On Ice found in the Bottoms of Rivers. By T. A. Knight, Esq. F.R.S. In a Letter addressed to the Right Hon. Sir Jos. Banks, Bart. G.C.B. P.R.S.—18. On the Action of detached Leaves of Plants. By T. A. Knight, Esq. F.R.S. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. G.C.B. P.R.S.—19. On the Manufacture of the Sulphate of Magnesia at Monte della Guardia near Genoa. By H. Holland, M.D. F.R.S.—20. On the Formation of Fat in the Intestine of the Tadpole, and on the Use of the Yelk in the Formation of the Embryo in the Egg. By Sir Everard Home, Bart. V.P.R.S.— 21. On the Structure of the crystalline Lens in Fishes and Quadrupeds, as ascertained by its Action on polarized Light. By David Brewster, LL.D. F R.S. Lond. and Edin. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. G.C.B. P.R.S.—22. Some further Account of the Fossil Remains of an Animal, of which a Description was given to the Society in 1814. By Sir Everard Home, Bart. V.P.R.S.—23. Further Observations on the Feet of Animals whose progressive Motion can be carried on against Gravity. By Sir Everard Home, Bart. V.P.R.S.—24. A new Demonstration of the Binomial Theorem. By Thomas Knight, Esq. Communicated by W. H. Wollaston, M.D. Sec. R.S.—25. On the Fluents of irrational Functions. By Edward French Bromhead, Esq. M.A. Communicated by J. F. W. Herschel, F.R.S.

Dr. Spurzheim has been for a long time preparing for publication a work on Insanity with the following title, "Pathology of Animal Life, or the Manifestations of the Human Mind in the State of Disease termed Insanity." Every person who is acquainted with the very distressing conditions of persons afflicted with diseasés of the mind, but particularly the insane poor who are confined for the security of society in the melancholy cells of a madhouse, must be glad to hear that any new light is about to be thrown on this hitherto very obscure and incurable disorder. The public attention, too, has been particularly drawn towards this subject of late, by the very intimate scrutiny made into public lunatic asylums by persons invested with authority, before a committee of the house of commons:—an investigation rendered absolutely necessary by the shamefully neglected state of insane persons, and the general ignorance of the causes and cure of the disease itself;—an investigation, too, which has left the public mind in a state of alarm for the treatment of their unfortunate fellow-creatures. Dr. Spurzheim, who has devoted many years of his life, and has exercised the most powerful talents, in pursuing this disease through all its stages and varieties, and who has spared neither time nor expense in visiting the principal asylums for the insane in Europe, has at length determined to lay his labours before the public, with the hope that, since such a considerable progress in the knowledge of the physiology of the brain and the manifestations of the mind in a healthy state has of late been made, a great deal may yet be done for those who suffer from its partial or general derangement, by a philosophical comparison of a very numerous collection of cases, with the peculiar organization and moral habits of the individuals. The work will be published in the course of a few months.

Several copies have recently been imported from Germany of that immense work on Meteorology, the *Ephemerides Societatis Palatinæ Meteorologicæ*, which contains abundant matter for the meteorological speculators, who have become so numerous of late in our country, to exercise their ingenuity on. It is remarkable that a work which contains so much information on a branch of philosophy now so very popular, should be so little known in Great Britain.

Mr. T. J. Pettigrew is preparing for publication, Memoirs of the Life and Writings of the late John Coakley Lettsom, M. and LL.D. F.R.S. F.A.S. F.L.S., &c. With a Selection from his Correspondence with the principal Literati of this and foreign Countries.

LXXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Nov. 7. The meeting of this Society after the long vacation was unfortunately much less cheerful than usual, in consequence of the indisposision of the President, who has so long given it life and vigour. A paper by Sir Everard Home (who was in the chair) was read, relative to the comparative anatomy of the *Lumbricus marinus* and *Lumbricus terrestris*, and comparing their structure and habits with the *Teredo navalis*. All these worms have red blood; all burrow in wood or clay; all have muscular stomachs, and their blood is aërated by tubes in their backs. The *Lumbricus marinus* has a small and scarcely perceptible organ, which Sir E. considers to be a heart, near the centre of its body, where the arteries from the head unite, and where the veins separate to supply the extremities. The author had in vain endeavoured to trace their organs or describe their bronchiæ, till aided by Mr. Cliff, who macerated the seaworm in vinegar; this coagulated its blood, and enabled him to make correct drawings of all its parts, and thereby ascertain the exact point where the arteries and veins ramified. The common earthworm has an artery along its belly and a vein along its back; in the latter are apertures to imbibe air.

Nov. 14. Dr. Johnson, by the hands of the President, furnished a paper On the Structure and Natural History of the Leech of Rivulets, which he calls *Hirudo vulgaris*, instead of *Hirudo octoculata* of Linnæus: his reason for adopting this name is the circumstance that the *Hirudo tessulata* has also eight eyes. The *Hirudo vulgaris* he describes as living under stones in rivulets, being from one inch to one inch and a half, is of a blackish brown on its upper side with spots and greenish underneath. It has no heart, but pulsates eight times in a minute. Dr. J. thinks that the whole of the leech genus are oviparous. This species copulates like snails, is oviparous, and the ova are quick in three weeks, and break the kind of capsule in which they are enveloped in about two months. It deposits from six to twenty or thirty ova, which are frequently destroyed by other leeches. The author detailed minutely the appearances which this leech affords in its various stages from the ovum till it is full grown.

Nov. 21. A paper by Dr. Philip Wilson, of Worcester, was read, relating some further experiments and observations on the effects of Galvanism in curing asthma. He stated that spasmodic asthma is a disease of very rare occurrence, and that Galvanism is of no advantage to it; but that in nervous asthma in
only

only one case in ten did it fail to give relief, and in most cases it was permanent." In galvanizing patients it was from five to ten minutes before they found their breathing relieved. He still used plates of only four inches square; began with only a few, and sometimes extended them to thirty. The conducting wires were applied to pieces of tinfoil placed on the nape of the neck and at the pit of the stomach, or rather lower, and he recommended the points of the wires to be moved a little on the tinfoil. He found that all patients could bear a greater power at first, than after they had been galvanized several times. Dr. W. assigned no reason for this fact; but it is evidently owing to the galvanic fluid blistering the skin on those parts, and thereby increasing their conducting powers, and at the same time augmenting their sensibility. In some habits it produces inflammation, and ulcers which continue open several weeks.

BELFAST ACADEMICAL INSTITUTION.

Tuesday, the 22d of October, an election for a Professor of Natural Philosophy came on in this Institution, when a number of gentlemen of high respectability and attainments offered themselves for the appointment. The candidates had all produced so many honourable documents, testifying their qualifications for the office, that it became difficult to point out the gentleman most capable of the appointment. After full deliberation, the electors proceeded to the ballot, when Mr. Knight of Aberdeen was found to have the greater part of the votes; and we have no doubt, from what we have heard, that he will do honour to himself and to the Institution which has chosen him to the important charge.

LXXX. *Intelligence and Miscellaneous Articles.*

STEAM ENGINES IN CORNWALL.

THE average work performed by thirty engines in the month of October was, according to Messrs. Leans' Report, 20,920,267 pounds of water lifted one foot high with each bushel of coals consumed.

Woolf's engine at Wheal Vor during the same month lifted 39,556,496 pounds; and the one at Wheal Abraham 50,698,188 pounds, with a load of 15·1 per square inch in cylinder, with each bushel. His other engine at the latter mine lifted 30,672,254 pounds with each bushel—her load 3·1 per inch in cylinder.

The engine at Wheal Chance, alluded to in our last, is reported to have lifted 44,615,811 pounds with each bushel, loaded 13·4 per square inch.   THER-

THERMOMETRIC SCALES.

To Mr. Tilloch.

Sir,—Referring to the note at the end of Mrs. Ibbetson's paper in your last number, I beg leave to observe that, not having immediate access to the work referred to in her preceding communication, I pointed out the error in the reduction of the degree of heat shown by the plant in Mons. Hubert's experiment, page 184, under the impression that Reaumur's thermometer was correctly quoted.

It now appearing that the circumstance is detailed with reference to Celsius's thermometer, it must be obvious that instead of making "the assertion respecting the heat the more impossible to be believed," it tends to strengthen it, by reducing the extent of the variation : at the same time that it shows an increase of temperature very evident to the sense of feeling, it brings the phænomenon more within the limits of probability.

In both Reaumur and Celsius's thermometers the zero is fixed at the freezing point of water, the range between that and boiling being 80° in the former, and 100° in the latter.

In my last* I pointed out that, according to the quotation in page 184, the degrees of heat shown by the standard thermometer and plant were

$$\frac{18\,R \times 9}{4} + 32 = 72\frac{1}{2}\ F. \text{ and } \frac{30\,R \times 9}{4} + 32 = 99\frac{1}{2}\ F.,$$ showing a rise of $27°\ F.$ Now as the degrees of Reaumur and Celsius bear a relation to each other of 4 to 5, the difference will be

$$\frac{18\,C \times 9}{5} + 32 = 64\frac{4}{5}\ F. \text{ standard thermometer}; \frac{30\,C \times 9}{5} + 32 =$$ 86 F. heat of the plant: the increase of temperature being $21\frac{1}{5}°$ F. which is still very considerable, and fully sufficient to produce the effect described.

I should apologize for intruding upon your time at such length on the present subject; but having in the former case pointed out what I considered an inadvertent error, I feel it in some degree incumbent upon me to notice that, although admitted, it has not been rectified.

I have the honour to be,

Your most obedient humble servant,

Thistle Grove, Old Brompton,   P. J. BROWN.
        Nov. 4, 1816.

* Mrs. Ibbetson's notice of the error alluded to having reached us, before Mr. Brown's, we judged it unnecessary then to insert the latter, as the thermometer referred to proved to be a different one from that which he noticed. His present communication embraces both.—EDIT.

M. de Bonnard, engineer of the French mines, has recently published in French a " Geognostic Essay on the Erzegebirge, or the Metalliferous Mountains of Saxony." The author has substituted the word *geognosy*, which is used by the Germans for that of geology, employed hitherto by the French, because he thinks it better adapted to express the science which is confined to the description of the nature and the peculiar arrangement of soils.

The country which M. de Bonnard has examined, comprises not only that part of the Saxon territory designated by the name of the circle of the Erzegebirge, but also a portion of the circle of Misnia, in the environs of Dresden, as well as the high mountains which separate Saxony from Bohemia, and some points of the latter kingdom, on the southern slope of the ridge. " It is one of the most interesting countries," M. de Bonnard observes, " of all those which a traveller can visit under a *geognostic* point of view ; it contains within a very limited space a great quantity of different soils ; the numerous mining concerns in full activity facilitate our observation : lastly, it is the constant theme of mineralogists, who visit it to learn from M. Werner himself the art of observing nature."

M. de Bonnard passed several months in Erzegebirge, and personally explored its stratification in company with some of the most distinguished mineralogists of the continent. He refers the whole constitution or arrangement of the Erzegebirge to three principal groups, each having a particular centre, and composed of rocks the arrangement of which has no connexion with that of the other groups, at least as to the primitive soils which constitute them essentially.

The first, which he calls *group* or *system of the east*, seems to be composed of rocks grouped around a nucleus of granite, situated near and to the eastward of Freyberg.

The second, which he calls *system of the south-west*, is composed of rocks one part of which is visibly supported on the granite of the north of Bohemia and the south-west of the Erzegebirge.

The third, to which he gives the name of *group of the north-west*, is formed almost entirely of *eurite (weigstein)*, which seems grouped around a hidden nucleus situated between the Zschoppau and the Mulda.

Between these groups of ancient soils we meet with more recent formations, which cover the slopes of the former and fill the intervals which separate them. M. de Bonnard describes separately those three systems of stratification, by examining for each of them successively the different kinds of soil which compose them.

them. He considers in the examination of each soil, 1st, the nature of the principal rock, and the intefesting geognostic facts which it presents ; 2d, the *subordinate banks* which this rock contains ; 3d, the veins (*filons*) which pass through it.

M. de Bonnard's arrangement confines him to the indications presented by the external form of the ground; and it has the advantage of presenting groups very clearly limited, and which differ from each other, either by the system of rock which is predominant, by the general inclination of these rocks, or by the nature of the nucleus around which they are grouped. Speaking of the anomalies which he had observed in the system or group of the east, M. de Bonnard thus expresses himself: " I ought to mention that the inclinations indicated are not without exception. Some anomalies are presented, for example, in the environs of Dippoldswalde, Glasshute, &c. ; others take place even in the vicinity of the granitic nucleus. The inclinations which are remarked in these places, different from those presented as general, may belong to some prolongation or hidden branch of the nucleus, or to other causes which are unknown to us : but it appears to me that they cannot shake the results drawn from the general observation of the observed arrangement of the soil."

M. de Bonnard speaks highly of M. Daubuisson's work on the mines of Freyberg, all now at work : they are upwards of 150 in number, and occupy 5000 workmen, producing annually 50,000 marcs of silver, a quantity of lead varying from 3000 to 10,000 quintals, and rarely more than from 100 to 200 quintals of copper. He also mentions, as a new and remarkable undertaking, the navigable and subterraneous canal called *Friederick-benno-stolln.* They have been obliged to dig this canal from Dorrenthal to Pfappenrode, a length of 1100 toises, to find the waters of a river (the Biela) in order to unite them with those which give motion to the machinery of Freyberg. It is intended to open another, also subterraneous, of 1600 toises, with the same view. Much more time will be requisite to finish these useful works ; but they will give the possibility of working the existing mines of Freyberg to more advantage, and of re-opening others which have been abandoned for want of the means of clearing them of water.

The principal shaft of the mines of Altenburg has been made by blasting in the stanniferous rock. The working always took place by blasting, by means of huge excavations, which were enlarged without any precaution, until in 1620 there was a general falling in of all the workings. This disaster produced an excavation of nearly 600 feet diameter by 300 feet in depth. They have continued to work by blasting in the parts which remained solid, and they have not given up the plan of chambers of large dimensions. These are dug in the parts most abounding in tin, those

which

which are nearly on a level are connected by galleries, and form stages to the number of six, which go to a depth of 140 fathoms. In old times they seem to have gone 35 toises lower. There exists an excavation which is 60 fathoms deep, and from 20 to 40 in breadth. M. de Bonnard regards these works as the most astonishing for their boldness which exist in any mine, but as by far too large, and therefore inconvenient.

M. de Bonnard terminates what he has said on the subject of the granite of his second system, with a few words on the hot springs of Carlsbad, a town situated on the Tœppel in a narrow valley shut in by two granite mountains, and which he conceives to be very extraordinary. In the valley there is no other species of rock, and it is from this basin that a spring of boiling water issues from under a calcareous arch which it has formed itself by its depositions, and under which is an excavation full of vapour and water, the depth of which is unknown. In order to account for this fact, M. Werner thinks that there is at the bottom of the valley, under the stratum of vegetable earth, coal in a state of combustion, which expands the water from the Tœppel, which filters through subterranean cavities. The waters of Carlsbad are the most frequented of Germany and Europe. They contain sulphate and muriate of soda, lime, carbonic acid, and iron : the sulphate of soda is the most abundant, and is extracted on the spot.

M. de Bonnard informs us that " in the mines of Joachimstadt there was found, at the depth of 150 toises, a large trunk of a bituminized tree, with the vestiges of bark, branches, and leaves. It has been entirely carried off piecemeal for mineralogical cabinets, and numerous specimens have been sold under the name of ' wood of the deluge.' "

In a very copious recapitulation he generalizes the observations made in the course of his paper, and pursues every separate rock through all the circumstances of stratification under which it has been presented to him.

GAS LIGHTS.

The advantages resulting from the application of gas to the lighting of streets and houses have been recently extended to the Surrey side of the Thames : and the whole of the Borough of Southwark, with that part of Blackfriars Road between the bridge and the Rev. Mr. Hill's chapel, is now brilliantly illuminated in this œconomical and efficient manner. In addition to the Surrey Institution, nearly all the shops in that district, and the elegant private residence of Mr. Potts in the Clink liberty, are lighted with gas, which is found fully to answer every purpose of domestic œconomy. The superb dining- and drawing-rooms, with the library

brary and billiard-room of this splendid mansion in particular, exhibit a most pleasing and brilliant scene when thus lighted; and Mr. Potts has added to the general effect by the introduction of an efficient system of ventilation, thus precluding all possibility of any disagreeable effect. The establishment from which the gas is supplied is situated at Bankside, and is under the direction of Messrs. Monro and Co. The premises are the most roomy that have been hitherto fitted up as a gas-light manufactory, and the whole arrangements of furnaces, retorts, and gasometers have been made upon the most correct and scientific principles. The utmost regularity is preserved in the various departments of the works, and not the slightest accident has occurred since their erection.

PREPARATION OF BORIUM.

M. Dobereiner has published a new process for extracting borium from borax.—After melting the borax and reducing it to a fine powder, one-tenth of its weight of lampblack is added; this mixture is put into a gunbarrel, one end of which is closed, and to the other is affixed a tube for receiving the gas, and the gunbarrel is then kept at a white heat during two hours. Much of the gaseous oxide of carbon is disengaged; and when the process is finished, there remains in the tube a compact mass of a blackish-gray colour, which is reduced to powder, and which, after being washed several times with boiling water and once with hydrochloric acid, yields a pulverulent greenish-black substance similar to borium, except that it is still mixed with a little charcoal. M. Dobereiner thinks that the charcoal begins by reducing the soda, and that afterwards it is the sodium which decomposes the boracic acid: consequently, he concludes that a greater quantity of borium might be obtained, if to the borax half its weight of soda or of potash were added, and a quantity of charcoal double of that which is here mentioned.

HERCULANEAN MANUSCRIPTS.

Messrs. Tyrwhitt and Hayter having been employed to unrol, if possible, the Herculaneum MSS. belonging to the Institute of France, by means of a new process, the Institute has appointed Messrs. Visconti, Quatremere de Quincy, Boissonade, and Raoul-Rochette, to superintend and assist in the operation.

To Mr. Tilloch.

SIR,—Being engaged in a baking business requiring a considerable supply of yeast, and not being at all times able to obtain a sufficient supply, I should consider myself much obliged if any of your well-informed correspondents would be pleased

to

to state the process of manufacturing that article,—the means of preserving the quality,—and what treatises in the English or French language exist on that subject, or on baking bread in general.   G. V.

[We recommend to our correspondent a perusal of the French work of the celebrated chemist M. Parmentier on the subject of baking.—EDIT.]

DRYING-STOVE.—OVEN.—STEAM APPARATUS.—CHEMICAL AFFINITY.

To Mr. Tilloch.

SIR,—I shall be much obliged to any of your intelligent read-ers, to inform me of the most œconomical and convenient mode of constructing a drying-stove on a small scale, where the heat required is above 212° Fahrenheit. What is the degree of heat applied in the drying of malt? and, that for the drying of wheat now much practised in consequence of its unripe state? What is the most œconomical construction for an oven for baking of bread, and to what degree of heat should it be raised before the dough be introduced? Is it important that it be air-tight for the baking of bread—and that a stream of air pass through for the cooking of meat? What is the most approved construction of an apparatus for heating liquids, and drying substances by steam, on a small scale, calculated for a laboratory or kitchen; and can such apparatus be obtained in London ready fitted up, and of whom?   O. C.

HYDROPHOBIA.

A Dublin practitioner states, that he has seen the symptoms of hydrophobia checked by the application of a tourniquet. A girl was bitten in the foot, and hydrophobia supervened; Dr. Stokes applied a tourniquet to her thigh, and the symptoms instant-ly subsided. His intention was to amputate the limb, but it was opposed by other medical attendants. One of this physician's pupils considers hydrophobia, like trismus, to be a spasmodic disease, which may be cured by the judicious application of tur-pentine with other antispasmodics. The effect of the tour-niquet seems to favour the plan of bleeding *ad deliquium.*

ARRIVAL OF SOME NON-DESCRIPT ANIMALS IN LONDON.

Four new and nondescript animals are now exhibiting in the King's Mews Riding-House: they seem to be an extraordinary species of deer, lately arrived from North America. The follow-ing is a sketch of their natural history.

As these new quadrupeds are natives of North America, which has now been discovered for more than three centuries past, it must necessarily excite wonder, that neither the horns nor the skin of such a fine animal, nor its description, have ever before reached

reached Europe, notwithstanding that America was discovered and settled, and is now occupied, by the descendants of Europeans.

The persons who have charge of these animals state, that a German naturalist, who had been employed several years in exploring that part of Louisiana, called the Upper Missouri country, brought them from thence over-land to Baltimore, where, as well as at Philadelphia and New York, they were exhibited for money.

They are in their nature very timid, and at the same time of such power and activity when grown, that it is not possible to take them out of the forest alive ; but some remote tribes of Indians having discovered that they were susceptible of domestication, and of being trained to draw their sledges in winter over the snow and ice, took them when fawns in nets, and brought them up in their houses with great care and kindness, thus depriving them of their wild habits, and making them at last of great value and importance for their services in harness.

Their flesh in the winter season is so juicy and nutritive that it is sought after with avidity by the White hunters as well as the Indians; in consequence of which, the species is threatened with an early and total destruction.

This animal is naturally inclined to be domestic. In his native abode he has his peculiar family or fraternity; each family has its own peculiar range of pasture, and does not intrude into that of its neighbours; he is not a rambler: and this family attachment is so strong, the hunters know that if they can knock down but one of them they can make sure of the rest at pleasure.

The name of this animal, in the language of the aborigines, is Wapiti, which has been adopted by Professor Mitchill, of the university of New York, and by the late Dr. Barton, of Philadelphia; but some naturalists have mistaken his character, and called him the Elk or Moose, which is an animal with broad palmated horns, and an uncouth figure, whereas the horn of the wapiti is round, and his figure elegant.

The age of the male of this species is ascertained by its horn till it is full grown; he sheds them annually: the females have no horns.

The colour of these animals is, in the winter, on the body, of a peculiar dunnish hue; the neck and legs are a dark brown, the rump is a pale yellowish white; the colour extending about six or seven inches from the tail on all sides, and very distinct from the general colour of the body. A black semi-circular line of unequal width (from a quarter of an inch to two inches) separates the white of the rump on either buttock, from the dun of the body.

The head resembles that of the common American deer (*Cervus Virginianus*)

Virginianus) and of the horse, much more than that of the elk
or moose, and is pointed and handsome. The legs are admirably
formed for strength and activity, resembling those of the race-
horse, particularly the hinder; on the outside of each of these is
a protuberance of yellowish hair, which is the seat of a gland that
secretes an unctuous substance, and the animal applies it to
smooth and dress his coat, which is so admirably constructed that
it is thus rendered impervious to rain, or to water if he swims
across a river.

· The wapiti has an oblique slit or opening under the inner an-
gle of each eye, nearly an inch long externally, which appears to
be an auxiliary to the nostril. He has no voice like the horse or
the ox, and this organ seems to be given him as a compensation;
for with it he makes a noise, which he can vary at pleasure, and
which is like the loud and piercing whistle that boys give by
putting their fingers in their mouth.

The anatomist and naturalist will find in the structure of this
animal a variety of objects highly deserving their best attention.

Notwithstanding the wapiti has a cloven foot and chews the
cud like the ox, yet he has a bridle tusk like the horse, and the
lower jaw is admirably fitted to be operated upon by a bridle and
bit. The tongue is remarkable for its softness and smoothness.

The wapiti is esteemed, and justly, the pride of the American
forest, being unquestionably the handsomest and most valuable
native quadruped that has yet been discovered in that extensive
country. He is mature when he is twelve years old, and his full
size is about eighteen hands. The largest of the two males now
in the King's Mews is full fourteen hands, and that he has but
lately entered his sixth year is manifest from his horns.

Like all other animals that are long in coming to maturity,
they live to a great age; the full extent of their lives is indeed not
perfectly known; but the Indians (who keep no registers) say of
a man, when he is grown in years and inactive, that " he is as
old as a wapiti," which certainly indicates that this animal must
at least equal the age of him to whom he is compared.

The two females appear to be smaller and somewhat younger
than the males; their necks resemble in some degree that of the
dromedary.

The food of the wapiti, in a domestic state, is the same as that
of the cow or the horse; and they are, if properly managed, equally
tractable. Ill usage or harshness makes them alarmed and impa-
tient; but they are very sensible of benefits, and lick the hand that
feeds them.

No quadruped can be more personally clean than the wapiti;
his breath is as sweet as that of the cow. The males are attached
to only one female, and the latter generally produce twins.

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The Upper Missouri country is in the same latitude as England; its winters are a little more rigorous and, its summers somewhat warmer; and it abounds in rich pastures of white trefoil, which the hunters call Buffalo grass.

From what has been stated, we find that these animals were exhibited as curiosities even in the principal American cities, consequently they must be unknown in all the cultivated and settled parts of the United States.

ALTERNATING COLOURS OF STARS.

To Mr. Tilloch.

SIR,—I HAVE lately made an observation on the rapid mutation of the colour of the light of some of the fixed stars, which I think worth noticing in your Magazine, in order principally to induce other persons to be attentive to the circumstance. The first observation of this kind I made was in the month of September, in a warm evening. In a star of the first magnitude I noticed a rapid alternation in the colour of the light. The change was from the ordinary colour to a deep red, and then back again to the common colour of the star. From repeated observations I found the red colour to last about two seconds, and the ordinary colour about three seconds.

That peculiar apparent motion in the light, commonly called twinkling, was very strong at the time. I observed this phænomenon during the space of above fifteen minutes, when a cloud obscuring the sky I could no longer see it. I attributed this to the refraction produced by some terrestrial vapour which might be rising up in undulatory or intercepted layers, and had no idea at the time of the possibility that it might depend on the manner of the transmission of light from the star, till I noticed it again this month in the α Orionis, when the reddish colour again alternated with the yellowish, and the alternation was more rapid and corresponded to the twinkling.

I have merely noticed this circumstance here in a hasty and cursory manner, to excite others to observe, as I have always noticed it in autumn. But I hope shortly to lay before the public more accurate minutes of observations on this phænomenon. Without offering any further comment on its possible cause at present, I am, in haste, yours, &c.

Clapton, Oct. 16, 1816.   THOMAS FORSTER.

P. S.—If any corresponding observations have been made in distant countries, I shall be obliged by their communication through your Magazine. I hope, with your permission, in a future number to give minutes of some of my observations which I have not by me at present.

TELLU.

TELLURIUM.

The following description of a new ore of tellurium, by Professor Esmark of Christiania, appears in the third volume of the Transactions of the Geological Society of London :—

It occurs coarsely disseminated, and crystallized in perfect hexagonal plates striped on the edges.

It has the whitish hue of tin.

Its fracture displays a perfect foliation, but only in one direction.

It has a strong metallic lustre; a moderate flexibility; considerable softness, and feels heavy.

Before the blowpipe it burns with the colour and smell of tellurium, and has the same action with acids. The precipitate by water burns upon charcoal like tellurium ; but after its sublimation a small metallic button remained of the colour of silver, malleable, but too minute for further examination ; for on account of the rarity of the substance I could only devote a single grain to this examination.

It is found in the Orndal copper mine, accompanied by copper pyrites, and a small intermixture of sulphuret of molybdenum. The mine is called Mosnap, and has been full of water for several years. The vein lies in mica slate. I met with the same ore in Hungary, in the collection of my friend M. Gerhard, who gave me a specimen of it ; but as he did not know from what part of Hungary it came, he did not value it. For this reason I omitted taking notice of it in the journal of my travels through Hungary and the Bannat of Temeswar.

It is easily distinguished from molybdenum, by its colour and its habitude before the blowpipe.

RUINS OF VELLEIA.

Parma, October 13.

Two Englishmen have been here some time, and have investigated with great attention our monuments of the arts, 'our museums, and our libraries; they visited the ruins of Velleia, and in assisting at the works discovered an elegant bas-relief which was placed over a fountain. It represents the two brothers Castor and Pollux. This bas-relief, which the celebrated antiquary M. De Lama, who is appointed to superintend the extrication of the ruins, believes to be Grecian, is in the best taste. It consists of four figures, and the attitude of Castor is admirable, having all the grace of the antique. These scientific Englishmen have had several conversations with Abbé Derossi, one of the most learned men in the Oriental languages, and who possesses an unique collection of bibles. The Emperor Alexander evinced his willingness to possess this valuable library; but it appears that Austria wishes this literary monument to remain in Italy. M. Derossi has refused the offers of those Englishmen, who wished to treat with him for his biblical library.

An unusual number of water-fowl, particularly wildgeese, have been observed early in this month in the marshes near the Thames and about Hackney: a circumstance which is said to denote a hard winter. ————

M̶r̶ Edward Forster of Clapton has recently found a variety of the curlew, *Scolopax arcuata*, of nearly one third more than the ordinary magnitude and with some variety in the colour, near Sandwich in Kent.

LATEST APPEARANCE OF SWALLOWS THIS SEASON.

Hirundo Apus—August 10.
———— *rustica*—October 11.
———— *urbica*—October 14.

LIST OF PATENTS FOR NEW INVENTIONS.

To William Losh, of Newcastle-upon-Tyne, iron-founder, and George Stephenson, of Killingworth, Northumberland, engineer, for their new method or methods of facilitating the conveyance of carriages and all manner of goods and materials along railways and frameways, by certain inventions and improvements in the construction of the machine-carriages, carriage-wheels, railways, and frameways employed for that purpose.—30th September, 1816.—2 months.

To Louis Fauche Borel, of Frith-street, Soho, for his method of making shoes and boots without sewing, so as entirely to keep out the wet, which invention may be applied to other useful purposes in leather.—25th October.—6 months.

To Lewis Grauholm, of Foster-lane, London, for his method or methods, process or processes, or means for rendering or making articles made or manufactured of hemp or flax, or of hemp or flax mixed, more durable than any such articles are as now made or manufactured.—25th October.—2 months.

To Benjamin Smythe, of Liverpool, schoolmaster, for his new machine, or apparatus, or a new method or methods of propelling vessels, boats, barges, and rafts of all kinds, and also other machinery, as mill-wheels and other revolving power.—1st November.—2 months.

To William Varley, of Hunslet, in the parish of Leeds, in the county of York, wire-worker, and Robert Hopwood Furness, of Bridlington, in the said county, soap-boiler, for their improved method of obtaining or producing saccharic matter or substance from wheat, rye, oats and barley, bear or bigg.—1st November.—6 months.

To Joseph Gregson, of Charles-street, Grosvenor-square, for his new method of constructing chimneys, and of supplying with fuel.—1st November.—2 months.

To

To Robert Ford, of Crouch End, in the parish of Hornsey and county of Middlesex, chemist, for his medicine for the cure of coughs, colds, asthmas and consumptions, which he denominates "Ford's Balsam of Horehound."—19th November.—2 months.

To George Washington Dickinson, of Great Queen-street, Lincoln's Inn Fields, in the county of Middlesex, for his new improved method, means, or contrivance, for preventing leakage from vessels employed to contain liquids, and for the preventing the admission of moisture into packages or vessels intended to be kept dry within.—1st November.—6 months.

To Simon Hosking, of the parish of St. Phillack, Cornwall, for his steam engine, on a new construction, for drawing water from mines, for working different kinds of machinery, and for other purposes for which steam engines are generally applied.—1st November.—6 months.

To William Day, of the Strand, county of Middlesex, for various improvements in or on trunks, and also in the application of certain machinery, by means of which they will contract or expand at pleasure.—1st November.—2 months.

To William Piercy, of Birmingham, for his new method of making thimbles.—1st November.—2 months.

To John Heathcoat, of Loughborough, for certain improvements upon machines or machinery, invented and in use for the purpose of making that kind of lace commonly known or distinguished by the name or names of bobbin-net or Buckinghamshire lace net.—1st November.—6 months.

To William Snowden, of Doncaster, for his new improved apparatus or machine to be attached or applied to carriages to prevent them being overturned.—1st November.—2 months.

To Robert Stirling, of Edinburgh, for improvements in diminishing the consumption of fuel, and in particular an engine capable of being applied to the moving machinery on a principle entirely new.—16th November.—6 months.

To John Day, of Brompton, for certain improvements and additions in the construction of piano-fortes and other keyed musical instruments.—16th November.—6 months.

To Robert Rains Baines, of Myton, in the county of the town of Kingston-upon-Hull, for his perpetual log or sea perambulator.—16th November.—6 months.

To William Russell, of Avery Farm Row, Chelsea, Middlesex, for his improvement upon cocks and vents for general purposes, particularly useful to brewers, distillers, private families, &c.—19th November.—6 months.

To John Barker, of Cottage Green, Camberwell, Surrey, for his improvement or improvements in the method or means of acting upon machinery.—19th November.—6 months.

To

T⸱ Walter Hall, of Serjeants Inn, London, merchant, in consequence of a communication from certain foreigners residing abroad, for his method or methods of making soft lead, and of hard lead, or slag lead.—21st November.—6 months.

T⸱James Kewley, of Aldersgate-street, London, for certain improvements in and on thermometers.—21st November.—6 months.

Meteorological Observations kept at Walthamstow, Essex, from October 15 *to November* 15, 1816.

[Between the Hours of Seven and Nine A.M.]

Date. Therm. Barom. Wind.

October

15	44	30·50	SE.—Foggy; gray day; star-light night.
16	43	30·50	SE.—Sun and hazy; fine sunny day; star-light.
17	44	29·75	N.—Foggy; damp and rainy till about 3 P.M.; star-light.
18	45	29·75	NW.—Rain and hazy; sun and *cirrostratus;* star-light.
19	37	29·80	SE.SW.SE.—Hazy; fine gray day; slight shower; dark.
20	44	29·60	NW.—Clear; wind and sun; cold wind and *cumulostratus;* shower 3 P.M.; sun; star-light. New moon.
21	44	29·66	NW.—Clear; sun and wind; gray day; star-light.
22	44	29·65	SW.—Foggy; fine day; stars bright.
23	32	29·95	SW.—White frost and sun; fine sunny day; star-light.
24	41	29·95	SE.—Clouds; sun and clouds; dark and rainy.
25	44	29·65	SE.—Hazy; rain 2½ P.M.; great shower and very dark; star-light.
26	39	29·65	SE.—Sun and hazy; very fine day; star-light.
27	51	29·66	SE.—Sun and wind; *cirrostratus;* very fine day; star-light. Moon first quarter.
28	52	29·66	E.—Cloudy; showers; some sun; moon, and star-light.
29	46	29·63	SE.—Hazy; clear and clouds; fine day; moon and star-light.
30	50	29·22	E.—Rain till near noon; sun and clouds; rainy evening.
31	45	29·20	SE.—Sun through fog; fine day; very black and great shower 2 P.M.; showers; moon and stars and hazy.

Date. Therm. Barom. Wind.

November

1 43 29·33 NE.—Hazy; fine day; slight showers; light, but no stars visible; foggy.

2 34 29·33 E.—Great rain till near 1 P.M.; some sunshine; very rainy till 8 P.M.; clear and *cumulostratus;* stars and moon; hazy.

3 45 29·33 SE.—Fine day; *cumuli;* clear at intervals.

4 43 29 66 E.—Gray; great shower at 7½ A.M.; showery; light, but no stars.

5 49 29·66 N.—Gray; slight showers and some sun; showery and damp; light, but no stars at night.—Full moon.

6 43 29·46 SE.—Clear and *cirrostratus;* foggy; clear and clouds; 3½ P.M. showery; moon and stars bright.

7 36 29·38 NW.—Sun and wind; very fine day; myrtles in bud, and also jessamines; great snow at 6½ P.M.; bright moon and stars.

8 24 29·50 N—S.—Sun and hazy; fine sunny day; snow on ground; clouds and moon.

9 42 29·00 SE.W.—Snow still visible, but soon went off; very fine day; slight showers; clouds; moon and stars; *cumuli.*

10 35 29·33 N.—Clear, clouds and wind; snow at 7¼ A.M.; very fine day; great snow at 6 P.M.; cloudy; ground and trees white with snow.

11 24 29·73 W—S.—Clear sun-rise; snow on ground; very fine day.

12 42 29·12 N—S. —Showery and windy; snow all disappeared; fine day; bright star-light. Moon, last quarter.

13 51 29·72 W.—Cloudy; very fine day; star-light and windy; wanecloud.

14 44 29·72 NW.W.—Clear, clouds and wind; very fine day; fine star-light, and windy.

15 29 29·50 W.—Clear and *cumulostratus;* very fine day; star-light.

⁕ A correspondent will be happy to have, through the medium of The Philosophical Magazine, any particulars relating to the Meteorolite reported to have fallen last autumn at Glastonbury.

METEOROLOGICAL JOURNAL KEPT AT BOSTON,
LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1816.	Age of the Moon	Thermo-meter.	Baro-meter.	State of the Weather and Modification of the Clouds.
	DAYS.			
Oct. 15	24	55·	30·30	Fair
16	25	58·5	30·12	Very fine
17	26	54·	29·90	Fair
18	27	53·5	29·90	Ditto
19	28	54·5	29·85	Ditto
20	new	49·	29·71	Ditto
21	1	49·	29·80	Ditto
22	2	51·	29·77	Ditto
23	3	48·	30·07	Very fine
24	4	51·	29·82	Cloudy
25	5	50·5	29·55	Ditto
26	6	49·	29·85	Fine
27	7	54·5	29·87	Fair
28	8	51·5	29·80	Showery
29	9	53·	29·73	Fair
30	10	50·	29·35	Cloudy—rained heavy all night and till 10 A. M.
31	11	52·	29·37	Fine at noon
Nov. 1	12	48·	29·40	Cloudy—heavy fog all day; rime frost at night
2	13	45·	29·50	Very fine—heavy rain in the even*
3	14	43·5	29·62	Rain
4	15	51·	29·85	Ditto
5	full	52·	29·84	Showery
6	17	50·5	29·49	Fair—rained hard in the evening
7	18	58·5	29·42	Ditto—gale from N. W.
8	19	35·	29·65	Ditto—rime frost
9	20	45·	28·95	Ditto—rain in the evening
10	21	34·	29·60	Ditto—heavy gale from N.
11	22	35·	29·75	Very fine—A. M. a storm of snow and rain with gale from S. W.
12	23	46·	29·60	Fair
13	24	52·	29·75	Ditto—heavy rain at night

The latter part of the month has been very tempestuous with variable winds. Much corn is still out in this neighbourhood.

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For November 1816.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.			
Oct. 27	55	57	56	29·50	32	Fair
28	53	57	49	·57	28	Showery
29	47	56	50	·50	31	Fair
30	50	51	50	·10	0	Rain
31	50	50	49	·21	0	Rain
Nov. 1	47	54	46	·30	27	Fair
2	39	48	47	·20	0	Rain
3	47	48	40	·41	26	Fair
4	46	51	47	·62	0	Rain
5	48	50	48	·58	0	Rain
6	48	49	40	·33	21	Fair
7	36	41	32	·32	28	Fair
8	29	38	42	·46	16	Fair
9	43	47	40	28·80	24	Fair
10	35	35	32	29·36	21	Fair
11	26	34	42	·40	20	Fair, snow at night
12	46	49	46	·56	24	Fair, violent storm [in the night
13	46	50	45	·62	20	Fair
14	44	47	35	·51	27	Fair
15	27	37	31	·42	28	Fair
16	28	40	30	·76	15	Cloudy
17	31	39	37	·95	26	Fair
18	42	47	40	·72	16	Showery
19	43	47	46	·75	14	Cloudy
20	46	51	45	·91	16	Fair
21	41	45	39	·76	26	Fair
22	37	38	31	·76	28	Fair
23	29	36	28	·80	29	Fair
24	25	30	26	·83	30	Fair
25	32	39	39	·82	0	Cloudy
26	45	44	40	·86	0	Rain

N.B. The Barometer's height is taken at one o'clock.

LXXXI. *On the Physiology of Vegetables.* *By Mrs.*
AGNES IBBETSON.

To Mr. Tilloch.

SIR, — How exquisite is this formation! how perfect this
scheme of plants!—Each day adds to the beauty of the system, to
the exquisite development of the vegetable, and renders the plan
still more perfect, more impossible to be confuted, as every part
exactly coincides.

In my last letter I showed that I was completely justified re-
specting all I had written concerning the flowers and leaves form-
ing in the root; and mentioned that Sir Wm. Herschel had done
me the honour to inspect and examine the specimens, and give
his assent to *this* (by far the most difficult part of the plan):
I then proved that the apparent flower consisted only of the
pistil and corolla, and that a specimen having these, if thrown
on a glass, would (from the line of life being *cut*) eject moisture
sufficient to continue the vegetable growth of the flowers as
long as the juices remained; and that I had known them con-
tinue in moisture sufficient, for nearly a whole week.

But I have now an additional fact of extreme importance to
communicate to the public, which most admirably continues,
nay, *finishes the foundation* of the growth of plants, in a manner
so complete, and with a simplicity *so exquisite,* as *at once* to
declare the plan of its origin to proceed from God alone, for no
human sagacity could have devised or suggested it. Before I
began my promised dissection of the buds, it occurred *to me*
(being the usual season in which to do it) to examine the seeds,
and see whether they could at all contribute to the showing the
flowers. But I had so carefully dissected them before, that I
despaired: but there is certainly a progressive line in my dis-
coveries over which I have no power. Words will not do justice
to my astonishment at finding that all the interior of the seed
(surrounding the embryo) is filled up either by bladders, which
each contain a separate *germ* tied together with an extremely fine
line of life, or branches of very diminutive size running all over
the interior of the seed, the embryo excepted. See fig. 1. Plate
No. 4, [Plate V.] a diminutive specimen of the part of the bean
surrounding the embryo. Fig. 2, the seed of the melon after
fructification. Fig. 3, the seed of the buckwheat. Fig. 4, the
interior of the large pea, a specimen surrounding the embryo
before fructification. This discovery led me to consider and to

conjecture, whether I should not find also a germ in the *heart* of *each seed*, while in the *root*, and mounting the alburnum vessel? since it must probably be in the *former* the seed would first take its shoot. But fancying that this must be a very difficult matter to see, from its extreme minuteness, I prepared my solar microscope with all its powers, and picked out the most *oily* of the *firs*, as well as a specimen of the *largest heart of the seed:*—the first in the *Pinus rubra*, the seeds of which are remarkably clear and full of bark juice; the second in the *Cactus*, which seed has an excessive large heart: and choosing a very sunny day, I at length discovered the germ, not only in the heart of the fir-seed while in the root, and in the alburnum; but more *obscurely* in the *Cactus*. The former, indeed, was very plain in six separate hearts, which I placed in one field of view. I perceived not only the germ to each, but the same species of root, or jelly-like *matter*, *before found* attached to the beginning sprout of the sea-weed, and imitated in the buckwheat, (fig. 3,) which appears also in the heart of the fir, (fig. 5.)

The regular process, therefore, seems to be—that the heart of the seed mounts with its germ from the root, where it takes it in, into the seed-*vessel*, and proves itself to be the seed not only by my being able to trace it through its whole progress to fructification, but by having the same germ in the heart it afterwards displays spreading from the primordial branch all around the embryo, after the seed is fructified: and thus, when the seed is fit to place in the ground, it is not only the embryo that is prepared to throw up its shoot, but the female flower is also fitted for its increase, and fixes, by means of the various germs in the seed, the beginning stripe for each line in the wood, (which I showed in my last letter,) and serves as a commencement also of flower the next year, in each flower-bud. The flower therefore, that is the cor and pericarp, is continued from plant to plant, while the male is renewed and reproduced yearly in every tree. This accounts for the bark and pollen in a graft never joining, while every other part is strictly assimilated. This proves also that I was right in saying that the stripes of flowers in the wood were formed *not the year they appear* at the exterior of the plant, but the *preceding:* indeed the appearance of the two is so dissimilar not only in form and size, but in situation also, that it is impossible to take them for each other; the first being simply in stripes, but those which are to open the present year, both in trees and herbaceous plants, are discovered not only larger, but in all sorts of *festoons*, *wreaths*, and *bouquets*, making the richest and most beautiful patterns.

Thus I may be now said to have brought the history of the

foundation of vegetable life to *some degree* of regularity and per-
fection. The germ which surrounds the embryo cannot grow
till after fructification, because the surrounding part of the heart
does not grow or increase till then; but there appears a little
germ round the vacancy in the heart below the branch from
which it seems *to shoot:* these germs always remain *white,* which
is probably the cause of their not being *before noticed;* for there
is such a confusion of richness (if I may so call it) as to render it
extremely difficult at first to distinguish or *discriminate each se-
parate ingredient;* and I have several times, in drawing the root of
the *Arum* more than twelve years ago, been astonished with seeing
flowers, leaves, branches, and seeds in quantities ; and though I
drew it *exactly* as it appeared, I was too prejudiced *then* to be-
lieve *what I saw* – I could not then credit that the received
opinions could be *so very far* removed from truth. Now I ven-
ture to trust my eyes, though still with proper diffidence I hope.
I have since found that in wet plants, in a dried specimen of the
arum root, and in the grasses, I can very plainly see the shoot
in the heart of the seed, while in *the root,* with *less powers* than
the solar microscope ; nay, with rather low ones of the com-
pound : yet it certainly requires good eyes, and the having (as
Sir William Herschel calls it) learned to see, to view it in all
its surrounding parts:—yet old as he is, so *perfect are his eyes*
that I am sure he would see it with even lower powers; for so
formed are his eyes by custom, so fitted for the purpose of
glasses, that they even now far exceed those of a young person.

I shall now give a sketch of the whole process, that I may be
thoroughly comprehended—begging pardon for repetitions that,
in so new a case, can hardly be avoided, without danger of mis-
conceptions.

When the seed is first formed in the extremity of the radicle,
it soon (from powder) changes to *balls,* tied together by a line of
life : for it is the collecting of this powder, and its attraction for
the *line,* which is probably the first cause of its aggregating.
The flowers then, which had spread from the *first old* seed, run
down into the thread roots and enter the radicle just where it
is fastened to the root ; and it must be THERE that each *heart*
of *the seed takes in its germ and female.* A curious speci-
men which I dissected *last year,* but could not then *comprehend,*
proves this:—it was a fresh grown radicle of the *Iris.* I make
it a *rule* to imitate exactly the pattern specimen (whether I un-
derstand *it or not*). I left it therefore, assured that Nature would
explain it *to my satisfaction at some future time;* which *it has
now done* (see fig. 6, AA), where the powder first forms into
seeds: it is in this plant of a pink colour, which makes it easily

distinguished :

distinguished. BB, where the powder forms into balls, *still co-loured.* CC, where the female or flower first enters the *radicle* from the root and old seed, and breaking off into shoots or sepa-rate germs they are drawn within the seeds. DD, a part where the pollen appears to be collected. The ball when first formed at AA, then consists of the heart of the seed only, composed of an empty bladder, and a shoot below it ; the vacuum is the part of the *corculum* which is first filled up, when the heart is fructified, and brought from the root; and the shoot is certainly the origin of the primordial branch, which reaches down from under the layers which form the embryo, and which when the embryo is completed runs under, to spread all over the interior surrounding part. When the seed is once fixed in the earth, it serves as a basis for the *stripes* of flowers in the wood, and as a commencement of those which entered the radicle*. But when the new-formed seeds are thus finished, they rise from the root, pass through the alburnum ; it is then the flowers of the *pre-ceding year* that enter the *flower-bud.* It is the festoons and bouquets that are alone forward enough to complete *them-selves,* and rise up by the *simple growth* of their several *pedun-cles,* and burst into *light* and *beauty.* No tree, therefore, could flower the *first year* ; and in annual and herbaceous plants, *sea-sons* must stand in lieu of *years.* It is by means of the com-parison of one part of the formation of plants we must judge of *another.* This is curiously exemplified in another case, as in the cabbage and many vegetables, whose yearly row of wood (instead of increasing each year) forms a new specimen *each season.* This may be the same with the stripes and bou-quets. Besides, in quicker-growing plants I much doubt whether this arrangement is not wholly managed by climate : at any rate the female flower as well as the line of life is the prolonga-tion of vital power; and the quantity of matter I have seen flow into the seeds at the time of the corolla passing off, and which I have often mentioned with astonishment, is *now* most admirably accounted for, as most probably forming the innume-rable new shoots of flowers in each separate bladder, surrounding *the embryo of the seeds just fructified.* Vegetables as well as animal bodies can convert the juices of the atmosphere into their own substance, and thus augment the number of their component particles. This operation is certainly as constant as

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* This indeed may be another reason why the seed is so long attached to the embryo and growing plant, not being severed by Nature till the germ has fixed its stripes in the wood of the new tree, and till the flowers have descended into the new radicles to enter the new-made seeds.—What exquisite contrivance! what admirable invention!

the exertion of that force by which they resist putrescence; for the absorption of atmospheric matter, its conversion into nutritive fluid, and the transmission of those juices of the body, *is in vegetables effected* by a constant absorption of the external surface. And in plants which increase indefinitely (for though I have been able to trace the stoppage of the yearly rows of wood, yet I never have seen the *yearly shoot miss*), they do not, like the animal body, lose on one side what they gain on the other. Still the internal parts change their condition : it is true the old and useless parts are thrown out, as I have shown in the new formed bark and wood; but the quantity of wood is still renewed in the new shoot each season, and the extension of the tree is increased, and increases in this way as long as life endures; though in extreme old age *it generally loses*, by wind and accidents, on the north and east sides what it gains by new shoots in the south and west.

However feeble and minute the parts of an embryo may be, we can certainly, in vegetables, trace it nearer the beginning of life than in animals: still it has its origin in the parent tree ; still it takes its line of life and female flower from the original seed, and from them receives its vital impulse; both have participated in the existence of other living beings, and exercised the functions themselves. Thus then the line of life and the female germ are the parts which *more particularly support and convey life in the vegetable,* and continue invariably from plant to plant; and, like all organized beings, are produced by generation, grow by nutrition, and are destroyed by decomposition.

I have noticed that at fig. 6, DD had the appearance of pollen between the seeds, being exactly like that which shows itself in the *alburnum.* There is still something obscure in the first growth of this powder, as I have never yet been able to say " *I saw it grow,*" as I have certainly and most truly done the seeds and flowers. Therefore there is something more to be discovered in this part: all that shows it *passing into the seedvessel* and *into the stamen,* is *as complete as investigation can make it;* but its first forming is defective: it is in the first shooting of the germ of a tree from its seed this must be *sought,* and probably its beginning may be traced in one of the inner folds of the radicle, both in the *tap root* and *side roots.* It will appear uncommonly stupid that, taking up a fresh tree every week or less, for four years together, I should not have traced all it contains. But I know not how it is, I never can attend to more than one subject at a time; and then all that may appear, besides the matter sought at that moment, is a *dead letter to me.* This is intolerably *foolish;*—still, perhaps, it causes me, by confining my attention, to fix it *more completely* on the

object

object of my study, and enables me to investigate *more deeply.* It certainly, however, makes my progress slower:—but I have 'been rather *accused* of doing *too much,* than *too little;* though I do not know how that can be the case, unless I have done carelessly and ill what I perform or discover.

Perhaps the next thing necessary to the obtaining a thorough knowledge of the formation of plants, and to comprehend their real *physical nature,* is to establish as exact an idea of the *situation* they hold in the animal and in the *inanimate world* as possible. I have already done this in some measure; but I still think a much more discriminating picture may be presented,— that by drawing the limits that in vegetables cannot be passed, and by fixing the boundary lines of such a being as a vegetable really is, will at once correct all those false ideas we are perpetually introducing, though so *foreign* to its *very nature.*

For example : As a plant has no brain, no nerves, or spine, no heart, it can have no *voluntary motion;* it can be moved by its *muscles only, that is,* by that *vis insita* it most truly possesses: it can therefore have no sensation, no feeling, for sensation and voluntary motion coincide ; nor, when compared with inanimate matter, can it be moved, as that is, by the introduction of caloric, by some mechanical agent separating its parts, or some chemical agent altering its combinations.

If therefore a motion is perceived in a vegetable, it cannot be attributed *to its air or its juices,* to which it is *but too commonly referred,* without considering that *such motion* belongs not to its *nature:* but the cause must be sought in that which is *analogous* to its *form,* which agrees with and appertains to its *life.* —Nor can the juices of the vegetable be said *to circulate,* since that is against the laws established for that species of being ; no animal or being having a circulation that is not possessed of a single *heart,* through which the whole mass of blood can pass, and in which it may be said to *centre.* Nor can the juices of any animal or being be said to *circulate,* where its juices are taken in or *respired* throughout the *whole* of their *surface.* Plants respire by means of their *hairs;*—many insects by *nearly the same* cause. " No animal," says Cuvier, " respires by *a particular organ,* except those which have a real circulation; because in them the blood coming from one *common source, the heart,* to which it returns, the vessels that contain it are so disposed, it cannot arrive at the other parts till it has passed through the lungs." This of course cannot take place in *vegetables, that have no heart ;* nor is there any place in plants at all analogous to that part. I have before given innumerable other reasons why this *circulation* (on which Mr. Knight so insists) is *impossible;* and the late discoveries make it still more

so.

so.   Nor can perspiration be more *incompatible* to the form and very *nature* of a vegetable.   No *cold-blooded animal perspires*, and plants may be considered in this light : a being whose juices never rise 10 or 13° above atmospheric heat, and whose blood, and that for a very short time, rarely deviates from it, may be called a cold-blooded being ; and can never require that perspiration necessary to cool and regulate the heat of the system.   The uses of perspiration are to free the blood from its redundant water,—an operation *vegetables* certainly cannot require,—and to expel from the body those *particles* which by repeated circulation have become *acrimonious*.   Now I have shown *before* that plants have no circulation, of course they require not this *reduction*.

But before I close this letter, I must add, that I have had fresh proofs of the growth of the *flowers on the glass ;* cutting seeds both before and after impregnation.   *The flowers* (when the piece of the seed has been left on the glass) *have spread in almost every instance* entirely all round it,—in one specimen at more than a quarter of an inch distance, in various patterns ; and that in two of the fir seeds, taken from the *root*, patterns have been prolonged evidently from the corculum of the seed.   (See fig. 7. and fig. 8.)

But I shall no *further* extend this letter, but leave to the opinion of my readers and botanists in general, whether the foundation of *botany* is not now so thoroughly delineated, that, without repeating every circumstance before discovered, I may without further prelude dedicate my future time to the consideration of the detail of vegetables, till I shall be able to lay the whole in one regular picture before the public, which I live but in the hopes of doing.   Every link of the chain is now complete, every division filled up.   This last discovery finishes the foundation, and renders it as perfect as human delineation admits, and as constant dissection and extreme labour will allow.'

I am, sir,

Your obliged humble servant,

Exeter, Nov. 2, 1816.   AGNES IBBETSON.

Description of the Plate No. 4, [see Pl. V.]

Fig. 1.   A diminutive specimen of the part of the bean surrounding the embryo.

Fig. 2. The seed of the melon just after fructification.

Fig. 3. The seed of the buckwheat taken round the embryo.

Fig. 4. The interior of the large pea, a small specimen preceding fructification.

Fig. 5. Specimens of the heart of the fir seed, now round, though afterwards a long oval ; taken while in the root, before
C c 4   - they

they enter the alburnum. They may be compared with the seed when it is first discovered in the seed-vessel, and will give positive proof that it was the *seed* in the root, since the same shoot will be discovered in both.

Fig. 6. The radicle of the *Iris*.

Fig. 7 and 8. Heart of the seed of the fir when divided, throwing out its germ or flower, and growing on the glass: it is impossible in this part to draw the flowers small enough to imitate even the magnified specimens.

LXXXII. *On the Origin of the Atomic Theory.*   *By* WILLIAM HIGGINS, *Esq.*

[Concluded from p. 371.]

THE next subject which the Doctor introduces, after giving my theory to Dalton, relates to the doctrine of Volumes discovered as he tells us by M. Gay Lussac, and published in the year 1809. He found " that they follow a very simple law in their combinations; that one volume of one gas always unites either with one volume of another gas, or with two volumes, or with three volumes." He then adduces a few examples to support this law.

" Gay Lussac also showed that the diminution of volume which takes place on the combination of these gases, follows as simple a law as the volumes in which they combine. In nitrous gas the oxygen and azote undergo no condensation: accordingly the specific gravity of nitrous gas is the mean between that of oxygen and azote. Frequently one of the two gases retains its bulk unaltered, while the other totally disappears."

" This curious law discovered by Gay Lussac, when coupled with the doctrine of Mr. Dalton, shows us, that there exists a very simple relation between the weight of the atoms and the bulk of the gaseous bodies. Accordingly, the weight of the atoms of these bodies may be deduced with sufficient accuracy from their specific gravity. This method was employed both by Sir H. Davy and Dr. Wollaston."

The theory of volumes of Gay Lussac was published after Dalton's Atomic Theory, therefore the latter could derive no advantage from it. Now the doctrine of the proportions in which particles unite to particles, or atoms to atoms, could only be deduced from a previous knowledge of the specific gravity of simple gases and of their compounds in the gaseous state, and of the proportions in which they unite volume to volume. The whole of my Atomic System was founded on those principles, and without this preliminary knowledge I could not advance a

single

single step. The Doctor acknowledges, in the number of his Annals of Philosophy for December 1813, that I anticipated Gay Lussac's theory of volumes :—*What a boon!* yet the Doctor never mentioned my name in the whole history of the Atomic Theory. This last circumstance shows a personal and rooted prejudice. It manifests in the most decisive manner a determined and wilful neglect, or rather suppression of previous labours.

The specific gravities of the different gases, known when I first wrote, were ascertained. There were very few added to them since.

The proportions in which they were capable of uniting in volumes were also known.

It was very well known that one volume of oxygen required two volumes of hydrogen to constitute water; and I had shown that the one and two volumes contained the same number of ultimate divisions, and that the difference of the specific gravity of those gases depended on the size of their respective particles, making an allowance for the more expanded state of the hydrogen gas. From this statement the weight of the atom of water was rendered very obvious.

The proportion in which oxygen and sulphuretted hydrogen, properly prepared, united in volumes, was also given.

It was stated, that oxygen does not contract by union with sulphur in any considerable degree, so as to produce sulphurous acid gas, and that this gas is nearly double the weight of the oxygen : hence it was inferred that an atom of sulphurous acid consisted of one ultimate particle of oxygen, and an ultimate particle of sulphur of the same weight. The quantity of oxygen consumed in saturating the sulphuretted hydrogen, gave on the above principles the relative number of the particles of hydrogen and sulphur in this compound gas.

It was very well ascertained long since by many chemists, that the gaseous oxide of azote contains one volume of azote and half a volume of oxygen; and I have shown that the particles of azote in this gas are united to a single particle of oxygen; consequently the half measure of oxygen contains as many divisions as a full measure of azote : and as the difference of the specific gravity of these gases is very small, we may infer that an ultimate particle of azote is nearly twice as heavy as that of oxygen[*].

* The difference between the weights of an ultimate particle of azote and oxygen was stated by me for the first time in my Atomic Theory. Dr. Thomson says, in the article under our consideration, that others made the same remark, without alluding to me. It might be the case; but I have not been able to trace it out. I shall always give full credit to the Doctor when he quotes the passage on what he asserts.

This

This gas is much more condensed than its constituent gases, and its specific gravity is in proportion to the degree of condensation.

Nitrous gas, whose atoms contain one of azote and two of oxygen, is much lighter than gaseous oxide. This struck me very much while writing my *Comparative View*, and in that work I attributed it to the size of the calorific atmospheres which surrounded its atoms. This gas consists of equal volumes of its constituent gases.

Nitrous gas and oxygen gas unite in the proportion of two of the former to one of the latter to form red nitrous acid, which contains one of azote and three of oxygen. This proves that there are as many divisions or ultimate particles in the one volume of oxygen as there are atoms in the two volumes of nitrous gas. This gives the weight of an atom of nitrrous acid.

When I wrote, I was well acquainted with the volumes in which carburetted hydrogen and oxygen united, viz. carburetted hydrogen 5·5 and oxygen 7·5; the quantity of this oxygen that was expended by the hydrogen of the gas was ascertained, and the remainder was found in the carbonic acid gas formed. Correct deductions were drawn from this experiment, viz. that carbon unites to two portions of oxygen. It was also ascertained that there were different kinds of carburetted hydrogen, and that they required different quantities of oxygen to saturate them *. The volumes in which azote and hydrogen united were ascertained by Dr. Austin and Berthollet, and the degree of their condensation were well known.

The volumes in which ammoniacal gas and muriatic acid gas, ammoniacal gas and sulphurous acid gas, ammoniacal gas and carbonic acid gas, unite so as to saturate each other, were well known to me, and to a few more in England, before I published on the subject of chemistry.

From the foregoing facts it is clear that there was nothing new in Gay Lussac's *important discovery of his theory of volumes*, as the Doctor is pleased to call it. If he could give it to his friend Dalton, he would not transfer it to a foreigner,—a foreigner no doubt of considerable celebrity in chemical science.

In this history of the Atomic Theory, the Doctor states that the celebrated Berzelius, having compared the quantity of oxygen in the base of a salt with the oxygen of its acid, found that they always bore a simple relation to each other. " They were either equal, or the oxygen in the acid was twice as much as that in the base, or thrice as much, or four times as much, &c. If

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* See Dr. Higgins's Experiments and Observations on Acetous Acid, &c. printed in 1786, p. 288. I studied with the Doctor at this time, and assisted in making all the experiments contained in that work.

the

the acid contained twice as much oxygen as the base, this was a proof that the acid contained two atoms of oxygen; if it contained thrice as much, it contained three atoms of oxygen; and so on. Thus in the sulphates the sulphuric acid contains three times as much oxygen as the quantity of base which it saturates. Hence we infer that sulphuric acid is a compound of one atom of sulphur and three atoms of oxygen. In the sulphites the acid contains twice as much as the base which it saturates. Hence we infer that sulphurous acid is composed of one atom of sulphur and two atoms of oxygen."

This is a singular mode of ascertaining the quantity of oxygen in the sulphuric and sulphurous acids.

I have proved, in my Comparative View, that sulphuric acid consists of one ultimate particle of sulphur and two of oxygen; and that sulphurous acid consists of one and one of its constituents.

I have also ascertained that a fresh-made solution of sulphate of iron contains in the molecule one ultimate particle of iron, three ultimate particles of oxygen, and one of sulphur, and that one-third of the oxygen was supplied by the water.

I will quote the following lines, taken from my *Comparative View,* on this subject *.

" When potash in solution is poured into a solution of sulphate of iron, immediate decomposition takes place, sulphate of potash is formed, and the iron is disengaged of a darkish blue colour, united with one-third the quantity of oxygen necessary to its perfect oxidation †. The iron could not receive this oxygen from the sulphuric acid being found united to the alkali in its perfect state, otherwise we should obtain a sulphite of potash."

When the solution of sulphate of iron is exposed to atmospheric air it attracts oxygen, and acquires a yellowish-brown colour. This oxygen must unite to the iron, the sulphuric acid having no affinity whatever to oxygen. During this process a yellow oxide is thrown down, which I thought at the time I wrote my Comparative View was occasioned by an union of carbonic acid to a portion of the oxide, in consequence of the weak affinity of the deutoxide of iron to the sulphuric acid. I also supposed that the acid of the oxide deposited must have been in a free state in the solution of the sulphate, particularly as the sulphate exposed to the operation was as neutral as a solution of a metal in an acid could be made.

But on inquiring into this affair more closely, I found that the

* See my Atomic Theory, pages 70 and 71.

† At that period (1788) I thought there were three oxides of iron; nothing has occurred since to induce me to alter my opinion.

oxysulphate,

oxysulphate, after the deposition of the oxide, remained as satu-
rated as the fresh-made sulphate itself previous to the change.
From the foregoing circumstance, it is probable that a molecule
of sulphate of iron consists of an atom of peroxide and an atom
of acid, and that the proportions of its constituent elements are
three of oxygen, one of iron, and one of sulphur; and that a mole-
cule of oxysulphate consists of an atom of deutoxide of iron and
two atoms of sulphuric acid; consequently its constituent elements
are four particles of oxygen, one of iron, and two of sulphur.
The foregoing facts do not clash with Berzelius's hypothesis of
the proportions of oxide in the base and acid of saline substances.
This yellow oxide dissolves in sulphuric acid diluted with a small
quantity of water in a moderate heat, and no effervescence takes
place; which proves that the yellow colour of the oxide of iron
is not occasioned by carbonic acid. This subject deserves more
attention and stricter investigation. The brown solution or oxy-
sulphate of iron possesses some properties different from the sul-
phate, it is no longer crystallizable, and when evaporated to dry-
ness the mass is deliquescent.

I observed in my *Comparative View*, that during the solution
of metals in nitrous acid they are first oxidized at the expense
of a portion of the acid itself, and that this oxide is held in so-
lution by nitrous acid, which is formed by a peculiar play of af-
finities during the chemical action of the ultimate particles of the
materials on each other, while at the same time nitrous gas is
evolved. The atomical demonstrations given on this subject in
my *Comparative View*, page 138, or page 126 in my Atomic
Theory, are worthy the attention of chemical readers.

I estimated this acid in metallic solutions to contain three of
oxygen, and the metallic base two. When a solution of potash
is poured into a solution of a metallic nitrate, it takes the acid
with one-half the oxygen from the metal, and the nitrate of pot-
ash thus formed, consists of the straw-coloured nitrous acid and
the alkaline base.

Other curious facts, which I noticed in my *Comparative View*,
on this subject, are worth relating here. " When iron is intro-
duced into nitric acid diluted with sixteen times its bulk of wa-
ter it is slowly dissolved, and azote instead of nitrous gas is dis-
engaged. Iron is dissolved in sulphurous acid, and there is no
hydrogen disengaged; if nitric acid be dropped in, the sul-
phurous acid is disengaged, and the nitric acid unites to the iron
without decomposition of any part of it, for no nitrous gas is
evolved*." These facts are accounted for on the atomic princi-
ple. The latter fact proves that the atom of nitric acid united

* See pages 127, 128, and 129, of Atomic Theory.

in

in toto to the ultimate particle of the iron, which afforded it at once oxygen of calcination and acid of solution. This is a proof that a molecule of nitrate of iron consists of five ultimate particles of oxygen, one of azote, and one of iron: or the quantity of oxygen might be estimated higher, if we suppose that the particle of iron was supplied with one particle of oxygen at the expense of a portion of the sulphurous acid during the solution.

This I am inclined to believe is the case, from the quantity of sulphur disengaged in the operation. The solution of nitrate of iron when exposed to the air is affected like the sulphate of iron; it acquires a brownish yellow colour, and a copious yellow oxide is deposited.

The red oxide of mercury is dissolved by nitric acid without decomposition of any part of the latter, and the molecule must contain seven of oxygen. These facts do not agree with Berzelius's doctrine of the relative proportions of oxygen in the bases and acids of saline bodies.

It was my opinion at the early period in which I brought forward the Atomic Theory, that an ultimate atom of a metallic oxide and an ultimate atom of an acid unite chemically, and that these were their definite proportions. I am still inclined to think that it is the case, with some exceptions, a few of which are adduced above: yet it would not be contrary to the laws which I established that atoms should unite 1 and 2, like the ultimate particles of elementary matter. We know that sulphate of potash will unite with two doses of sulphuric acid, that potash will unite with two portions of acid of tartar, that potash will also unite to two doses of oxalic acid, although the second dose is so strongly attached in the above salts, that nothing but chemical means will separate them: yet it appears to me that it is not a chemical union, particularly as those saline substances have an acid taste. It is probably a kind of intermediate influence, somewhat similar to what exists between saline substances and water, or sugar and water, and gases and water, or alcohol and water; and the union which takes place between two neutral salts is of a similar nature.

The supposition that two atoms of one substance will unite to three atoms of another substance is contrary to the laws of the Atomic System, as I have shown in my *Comparative View*, so far as relates to the combinations of the ultimate particles of elementary matter; and I should imagine that the same law extends to the combination of atoms, for atoms are as distinct or insulated from each other as particles. Therefore I am inclined to differ from the calculation of Dr. Thomson, that according to the analysis of Berzelius of the sulphate of iron, it consists of two atoms of acid and three of oxide. Now one atom of the

acid

acid unites to one atom of oxide ; the second atom of acid will attach itself to another atom of oxide, so as to form two distinct molecules:—how is the other atom to be disposed of ? It cannot divide itself between the two molecules : therefore there must be an error in the analysis or in the calculation, or perhaps in both. An atom of acid can no more unite to two atoms of its base, than a particle of oxygen can unite to two particles of azote, or to two particles of hydrogen, although an atom of the base may unite to two atoms of acid.

But to return to the history. "After the preceding historical sketch," continues the Doctor, " which we thought necessary in order to do *justice* to *all* parties concerned in this important branch of chemistry, we shall proceed to lay the Atomic Theory with the proofs in support of it before our readers *." The Doctor then proceeds to give the outlines of the Atomic Theory; in which it is needless to follow him, for it is a reiteration of my own doctrine, the outlines of which I have given above.

The Doctor concludes his article by giving tables,

1. Of the weight of the ultimate divisions of elementary bodies.

2. Of atoms, and the number of ultimate particles they contain.

3. Of molecules of three elements, such as those of the vegetable acids. This is taken from the analyses of Berzelius. Next come the hydrates, and after them the whole tribe of saline bodies, all weighed, and the number of their elementary particles ascertained. His estimate is taken from the analyses of different chemists, but principally from those of Gay Lussac, Thenard, and Berzelius. Thus the Doctor has weighed almost all the different kinds of ultimate particles, atoms and molecules, which the surface of the globe affords. What is to become of Dalton, for he has not left him a single atom ? It was very unkind to take from a man what he discovered himself; and if Dalton had not given them ready made to the Doctor, how could he weigh them ?

I felt the importance of the Atomic Theory in my investigation of the contending doctrines already alluded to, and it was on that occasion that necessity gave it birth in my mind.

Berzelius availed himself of the lights this doctrine afforded, in his analyses of saline substances and vegetable materials; yet he never claimed any part of the doctrine as his own.

Mr. Dalton, in his work, claims the whole of it in the iden-

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* The reader, no doubt, with a moderate share of chemical knowledge, will be able to judge, from the short sketch which I have given, of the kind of justice he has done " *to all persons concerned.*"

tical form in which it originally appeared, except that of changing my diagrams into symbols, and leaving out the numbers in my diagrams, the importance of which was already explained. This omission was, no doubt, in order to disguise ; and the same omission is still continued for the same purpose. It is much to be lamented that this beautiful doctrine should be mutilated of its best feature, in order to conceal the marks by which its rightful possessor might have laid claim to his property. During my investigations in my *Comparative View*, I could accomplish nothing of any consequence without a knowledge of these relative forces. In short, they must be restored to the science, in spite of all interested attempts at their suppression.

Dr. Thomson steps forward as the advocate of Mr. Dalton, while he himself stands trembling and silent at the bar of justice. Unfortunately for the Doctor, he embarked in a very unjust cause which will never afford him credit, but draw on him the disapprobation of every friend to justice and liberality.

What prevented Mr. Dalton himself from coming forward in his own defence, when Sir H. Davy, very honourably, first announced to the chemical world that I was the original author of the *Atomic Theory*, or when I published my Atomic Theory in 1814, which claimed that doctrine; or when Dr.Crean, of Boston in England, took notice of it in the Philosophical Magazine, some time before the above work was written ?

The Doctor in defence of his friend, in his Annals of Philosophy, asserts many things which he could not substantiate ; and when he found that Dalton must renounce his pretensions to the Atomic Theory, or that of definite proportions, he endeavours to tear it in pieces, and to divide it between Bergman, Black, Cullen, Fordyce, and the *Encyclopédie Méthodique.*

It was incumbent on the Doctor to produce extracts from those authors, in order to prove his positions. This no doubt he would have done very readily were it in his power.

But in the above distribution he takes care to reserve a remnant for his friend Dalton ; that is, the weighing of atoms and molecules which I cut out for him, with the due proportions of their respective elements.

The Doctor takes great pains, vol. iv. p. 54, of his Annals of Philosophy, to show that my Comparative View was very little known or read. The following are his observations, in his own words : " As far as I have had an opportunity of judging, Mr. Higgins's *Comparative View* was very little known to chemists in general, till Sir H. Davy published his note, claiming for it the discovery of the Atomic Theory. I myself met with a copy of it by accident in 1798. I never met with a single person in Edinburgh who had read it: nor were any of the London chemists.

mists, as far as my knowledge goes, acquainted with it, before Mr.
Higgins pointed it out to Sir H. Davy as containing the outline
of the Atomic Theory."

I will now pass over part of the quotation, and come to the
conclusion of it.

"For my own part, as I have already said, I met with a copy
of the book by accident, in the year 1798, when I was a student
at the University of Edinburgh; I read it at that time in a cur-
sory manner, and never looked at it again till Davy's note ap-
peared. I then read it again; and told Davy at the time first
I could not find the Atomic Theory in it. I put necessary in
soon afterwards both to Dr. Henry and Mc in this important
of them assured me they never saw who lay the Atomic Theory

Now Dr. Henry published his before our readers *." The
long before Dalton laid hold the outlines of the Atomic Theory;
from my *Comparative* w him, for it is a reiteration of my
of his book.   which I have given above.

The same quotation is given in his fifth edition of the same
work, published in 1805, page 163.   In short, he quotes from
my Comparative View in every work of his that I have seen.

How will the Doctor get over this ?—what could be his motive
to advance such an erroneous statement ?   It was evidently to
defend Dalton against the suspicion of plagiarism : for he was
aware, if Henry should be known to be acquainted with the
work, that Dalton his neighbour and intimate friend could not
be a stranger to it.   My Comparative View passed through two
editions in the course of two years, although it was printed and
published at my own expense; therefore it could not be so much
neglected as the Doctor wishes to intimate.   But suppose a sin-
gle copy of it had not been sold, it would not take away from
the merit of the work.

But the most remarkable feature of the Doctor's conduct on
this subject is, that after having endeavoured to give every part
of the Atomic Theory which is registered in my work to those
authors which I have enumerated, he at once transfers it back
again to Dalton, in his history of the Atomic Theory as the fun-
damental principles of estimating the weight of atoms and mole-
cules.

What inconsistency! what prevarication !

I claim nothing but what appears in my Comparative View ;
and it is very evident that the definite proportions in which ele-
mentary particles unite are there stated for the first time : and
I defy the Doctor to adduce a single instance to the contrary,
from any work that appeared before or after mine, except that
of Dalton's.   Then the simple question is—to which of us it be-
longs ?

But

But the Doctor endeavours to persuade the public that the
doctrine consists in the calculation of the weight of particles,
atoms and molecules, and that I had no conception of such a
doctrine.

Thus the Doctor craftily endeavours to throw the fundamental
principles of the system into the .ick-ground, while he brings
forward its effects or natural consequences as the theory itself.
Any person that reads my Atomic Theory will readily perceive
+' attended not only to the relative weight of the gases, but
notu. a that of their elementary particles; and the doctrine of
forces. In roportions could never be brought forward by any other
all interested atte.u.ready observed.

Dr. Thomson steps for in defiance of the most glaring evi-
while he himself stands trembling to strip me of the doctrine in
Unfortunately for the Doctor, he en. llest doubt but his efforts will
which will never afford him credit,
probation of every friend to instie . . to his Annals, I
sh. t make n..nothing but misrepresentations in a great
national work could in luce me to step forward ; and it is to be
hoped that the proprietors of that work will, in their next volume,
or in their next edition, correct the errors and prejudices in the
article in question, for in its present state it is a disgrace to
their pages.

As my *Comparative View* has long since been out of print, I
refer the reader to my *Experiments and Observations on the
Atomic Theory and Electrical Phænomena,* where the whole of
this affair is more fully stated. It is sold by Longman and Co.,
London.

I am, sir,
Your very humble servant,
Wm. HIGGINS.

Dublin Society House, Dublin,
Nov. 13, 1816.

LXXXIII. *An Account of the Discovery of a Mass of Native
Iron in Brasil. By* A. F. MORNAY, *Esq. in a Letter to*
W. H. WOLLASTON, *M.D. Sec. R.S.** *

DEAR SIR,— NEAR five years have elapsed since I presented
you with a specimen of native iron from Brasil. Particular
reasons prevented me at that time from making it more gene-
rally known; and since then my private affairs have not allowed
me a moment to look into my notes, and give you this short
account of the block from which your specimen was cut,
though I have so often promised it you.

* From the Transactions of the Royal Society for 1816, part ii.

In the autumn of 1810, I discovered near Bahia a spring of water strongly impregnated with iron, which was esteemed a most valuable acquisition in that country. This circumstance called to the recollection of the government, that, about thirty years before, information had been received of the discovery of certain thermal springs, situated at the distance of forty or fifty leagues to the northward; and as His Royal Highness the Prince Regent of Portugal had inquired, during his stay at Bahia, whether the country possessed any thermal waters, I was requested to visit the spot where they were supposed to exist. The governor-general offered me every facility and protection; and in order to induce me to undertake the journey, some of my friends described to me an extraordinary *stone* which had been found still further up the country, in the same direction. It had been supposed to be silver or iron, or that ferruginous agglomeration so common in Brasil, which often envelopes gold, and I believe sometimes diamonds. On the other hand, some persons who pretended to have seen it, asserted that it was not a mass of any metal, but had only the metallic sound on being struck, common to numerous blocks of stone in the same neighbourhood, called by the inhabitants " serpent stones," in consequence of their exfoliating by decomposition at the surface. As the serpent casts his skin yearly, so they suppose these stones to do.

Some account of the discovery of this extraordinary mass had been given to the government of Bahia, and through the inspector-general of the militia, a man of great talents and considerable learning, I obtained a sight of the papers on the subject existing at the Government-house. On reading them, I was decidedly of opinion, that the mass described was native or meteoric iron, and I determined to go to see it. But before I relate my own observations, I will give you the substance of the notes which I took out of those papers.

In the year 1784, a man of the name of Bernardino da Mota Botelho, while looking after his cattle, noticed the block in question, as being different from all the other stones on the spot, and informed the governor-general of the province of Bahia of his observation. His excellency immediately ordered the head man of a neighbouring village, that is to say, at the distance of near fifty leagues, to go and examine it. He did so, and reported very marvellous things, calling the mass sometimes iron, and sometimes stone, but giving to understand that it contained gold and silver. The governor-general commanded him, in consequence, to have it conveyed to Bahia. This man returned to the spot, and after having excavated round the block, so as to be able to get the ends of four powerful levers under it, he

contrived

contrived by great exertion, with the assistance of thirty men, to turn it on its side. He observed the bed on which it rested to be of the same scaly substance that was attached to the bottom of the mass, and about eighteen inches thick.

About the latter end of 1785, he conveyed to the spot a waggon, or rather a truck built for the purpose, and succeeded in getting the mass of iron into it; but having spent three days in this operation, the men employed were obliged to depart, in consequence of the neighbouring rivulet being brackish, and not fit to be drunk. They returned, however, and yoked oxen to the truck, but they could not move it until they had put on twenty pair of oxen on each side. You must observe that their oxen are not of the strength of ours, that the ground was a loose gravel, and that the truck was constructed on the very worst plan, the wheels being fixed to the axletrees, and the two axletrees remaining constantly in a parallel position with respect to each other.

They proceeded, however, in this manner to the distance of about one hundred yards, when they got into the bed of the rivulet above mentioned, called the Bendegó. There it was stopped by the prominent point of a rock; and as the truck was only calculated to move in a straight line, it was abandoned.

I visited this mass on the 17th of January 1811, and found it still on the waggon or truck, where it had been lying for five-and-twenty years. It is situated near the left bank of the rivulet, but entirely in its bed, which was then dry, and is very seldom otherwise.

I send you a very correct outline of this mass. (Pl. V.) It is about seven feet long, four feet wide, and two feet in thickness, besides a sort of foot on which it now stands, of about six inches in height. The solid contents, however, cannot be inferred correctly from these dimensions, since the broad part is hollowed out underneath very considerably. After making due allowance for the cavities, I estimated on the spot, the solid contents of the whole mass to be at least twenty-eight cubic feet, which at 500 lbs. will make its weight to be 14,000 lbs.

Its colour is exactly that of a chesnut, and is glossy at the top and sides, but the hollow part underneath is covered with a crust in thick flakes, outwardly of the colour of rust of iron, and staining the fingers. The flakes are very brittle, and the fresh fracture is black and brilliant, like some magnetic iron ores.

The glossy surfaces of the block are not smooth, but slightly indented all over, as if they had been hammered with a rather large round-headed hammer.

There are several cavities in it, from the diameter of a twelve-pound cannon ball, to that of a musket-ball; the larger ones being

D d 2 • shallow,

shallow, but the others much deeper. They all contain the same substance as is attached to the great cavity underneath, and some of them also fragments of quartzous stones, which I was obliged to break in the holes in order to get them out.

The brown colour of the surface of the block is merely a very thin coat of rust, for the slightest scratch with a knife produces a bright white metallic streak ; and yet, wherever the mass is struck with a steel, it gives out sparks abundantly.

When rubbed with a quartzous pebble in the dark, it becomes beautifully luminous.

The block is magnetic, and even possesses well marked poles. In the outline I have indicated their position. The N. pole is not so well characterized at the shorter point of the same end.

The N. pole of the block lies at present nearly E.N.E.; before it was removed it lay about N.N.E. I ought to tell you that La Mota Botelho, who first noticed this object, accompanied me, and, as he was present at its removal, he was able to give me much information, being a very intelligent man.

The N. pole is by much the most massive end, and lay deeper in the ground than the other.

No part of the mass has the power of attracting iron filings, whether the spot have been filed to brightness or not.

I had provided myself with a sledge hammer and tools for cutting off some specimens of the iron, but it was with the utmost difficulty that I could detach the few small pieces which you have seen, one of which I gave to you on my arrival in England. The largest I presented to my Lord Dundas, to whom I am under many obligations, and who promised to place it in the collection of the Geological Society. I also presented fragments to our lamented friend Mr. Tennant, and to Dr. Marcet. Another specimen, beautifully crystallized, I disposed of to Mr. Heuland, and I have only some small pieces left. As soon as the first piece was detached, I was struck with the appearance of internal crystallization not hitherto noticed in meteoric iron ; but as your specimen shows this circumstance very well, I need not describe it.

None of the fragments possess magnetic poles.

No vitreous substance appears about the mass, as in many of the known blocks of meteoric iron.

Having taken a few reagents with me, for the examination of the thermal springs which had been pointed out to me, I tried the malleable part of the mass on the spot, for nickel, and I thought at the time that its presence was indicated ; but I am now satisfied that the phænomena which I noticed, might have arisen from iron alone.

I have found my specimens more liable to rust, I think, than
wrought

wrought iron generally is; and in a damp atmosphere a liquid oozes out from the crevices.

I repaired to the spot where the mass was discovered, namely on a rising ground on the left bank of the river Bendegó, and caused the soil and gravel to be removed until we came to the bed described in the government documents. We found it at less than three feet depth. I had expected to find in it a considerable protuberance, such as might have fitted the cavity underneath the mass of iron, for I was convinced that the block itself must have been firmly attached to the bed, otherwise it would not have required such a considerable power to turn it on its side.

However, I did not; and thinking that we were not exactly on the spot, I caused two trenches to be opened down to the bed, and crossing each other, the one being between two and three yards long, and the other between one and two. Every part of the bed that was uncovered was perfectly flat and horizontal, except where we dug first; there it was broken, and, according to the statement of La Mota Botelho, that was done when the block was removed.

I found no termination to the bed in the directions of the trenches, and at the spot where the mass had lain, it was about one foot thick, or hardly so much; but at one end of the longer trench, not above three inches. I did not break through it any where else. Nearly the same loose gravel appears underneath the bed as over it. I brought away specimens of the bed, which I considered extremely curious, supposing them to contain nickel. On my return to England I told you, therefore, that I hoped I had found iron ore containing nickel; for I thought that the bed, on which had rested the mass, was one of those of which there are so many all over the province. But as I gave you some specimens, I will not describe it.

The surface of the soil, or rather coarse gravel, at the spot, is about ten or fifteen feet above the main granite rock of the country.

I can only give you an approximation of the latitude and longitude of the place. The sun was much too high at noon to take its altitude with a sextant and mercurial horizon; and the artificial horizon, which I had been compelled to construct myself, occasioned such a loss of light, as to make it impossible to observe the southern stars for determining the latitude. Different altitudes of the sun at a distance from the meridian, did not give me satisfactory results. I had with me an excellent watch, and having computed the latitude to be about 10° 20′ S. I concluded the longitude to be 33′ 15″ W. of Bahia, after making every allowance, and comparing this result with those

D d 3   obtained

obtained before and afterwards, at the house of Major Dantas, called Camuciatá, near Itapicurú.

The rapidity of growth in plants is wonderful in the neighbourhood of the Bendegó, although the main granite rock is so near the surface as to protrude in many places; and what lies on it is chiefly a coarse gravel, consisting of rolled fragments of quartz, felspar and granite of the size of eggs, together with smaller pebbles and sand, which contains, of course, a great deal of mica, but hardly any vegetable earth.

At about forty leagues to the southward, are found hills of yellow and red sandstone, in which organic remains have not been found; while to the northward there is a formation of similar hills, in which are observed most beautiful impressions of whole fishes and remains of vegetables.

Between the Bendegó and the sandstone hills to the southward, I observed a great deal of what I certainly take to be basalt. I met with balls from the diameter of two inches to that of upwards of three feet, and numberless prisms, with three and with six faces, scattered about; all of these small, that is to say, about three or four inches in length, and two or three in diameter.

To the southward of the sandstone hills is a sandy plain, almost barren, extending many miles, perhaps sixty or eighty, east and west to the sea, but not twenty in breadth, where I crossed it. Small conical hillocks are scattered over it, some of which, the largest, have flat tops, and appear all to be of the same height, about twenty fathoms.

Appearances impressed me with the idea, that they were the remains of a plain which formerly extended over the one on which I then stood, but which had been washed away in a tumultuous manner by a violent current running nearly in an easterly direction. The larger hillocks appear to be stratified, but they consist of loose sandy materials, except in so far as they contain beds of a dark-red iron ore, containing imbedded minute crystals of magnetic iron ore : the thickness of these beds is about two inches, and they are exactly similar to those which are found in the clay hills of Bahia.

The smaller hillocks consist of confused heaps of gravel and loose stones, intermixed with a very large quantity of the same iron ore in fragments, and lumps of manganese, very compact, and of a steel-gray colour, containing arsenic, but apparently no iron.

The dreary appearance of this plain is increased by the numerous nests of *cupim* (white ants) standing upright like so many tombstones. On being viewed nearer, they are conical, rather compressed, so that the base is elliptic. All those which

I examined

I examined were precisely of the same shape. The materials of which they consist are white sand, whitish clay, and particles of wood.

Many of them were full five feet in height.

The soil of the valleys and low grounds, which are occasionally swampy, is abundantly impregnated with sea-salt, which the inhabitants wash out for their own consumption; but it contains some bitter salts, which render it purgative to those who are not accustomed to it.

The thermal springs which were pointed out to me, were several, but they hardly deserve the name.

One of them was at 86° of Fahrenheit when the atmosphere was at 81°.

Another was at 88°, when the atmosphere was at $77\frac{1}{2}°$; and also at 88°, when the atmosphere was at 80°.

The water of both of these is the purest I had ever seen. Many small fish were swimming in the basin of the last, from which runs, at all seasons, a considerable rivulet.

A third was at 90°, when the atmosphere was at 73°. The water very pure.

A fourth was at 101°, when the atmosphere was at $85\frac{1}{2}°$; also at 101°, when the atmosphere was at 93°.

Taste of the water rather ferruginous, and very brackish, extremely disagreeable and nauseous. No peculiar smell, and very transparent, although it deposits iron and lime, and an iridescent film is formed on its surface. Contains no sulphuretted gas. The rocks of the neighbourhood contain pyrites not magnetic.

This spring is called the Mai-d'agoa, and is situated on the left bank of the river Itapicurú, near the water's edge, at a short distance from a place called the Mato-do-cipó.

It was during this journey that I had an opportunity of seeing that curious plant called *Cipó de cunanam.* It grows abundantly between Monte Santo and the river Bendegó. It is a climbing plant destitute of leaves; it was so when I saw it, and I believe it to be always the same; it bears no thorns; but often growing so as to form an impenetrable *plica* which the cattle will hardly approach, much less attempt to break through, because when the juice of this plant sticks to their hair, it occasions blisters and great irritation. It contains a milky juice, and I suppose that it is an *euphorbium.* When I made a cut at the bush with my hanger, in the dusk of the evening, the wounds inflicted presented a beautifully luminous line, which was not transient, but lasted for several seconds, a quarter of a minute. Having taken a piece of the plant, I bent it in the dark until the skin cracked, when every crack showed the same light, which is of a phos-

phorescent

phorescent appearance. I continued to bend the twig until the milky juice dropped out, when each drop was a drop of fire, very much like what I have seen on dropping inflamed tallow. I did not observe any particular smell. The milky juice is said to be very poisonous; it is caustic, and occasions much itching and irritation when applied to the skin. It becomes viscous in the air, and soon dries of a yellowish colour, slightly tinged with green, when it has the appearance of a gum-resin.

The above account contains all the information that I can give you on the subject:—should you think it deserving to be laid before the Royal Society, I would beg of you to add your observations, as they would render the communication interesting.

      I am, with sentiments of the highest esteem and respect,
            My dear sir,
                Your faithful friend and devoted servant,
London, April 27, 1816.   A. F. MORNAY.

    To Dr. Wollaston,
Secretary of the Royal Society.

LXXXIV. *Observations and Experiments on the Mass of Native Iron found in Brasil.* By W. H. WOLLASTON, *M.D. Sec. R.S.*[*]

THE preceding letter from Mr. Mornay, relating to the discovery of a mass of native iron in Bahia, was drawn up at my request, as a valuable addition to our stock of knowledge on that most curious subject; and I am in hopes that the results of my own experiments may contribute something not uninteresting to the Society.

The specimen of the iron with which Mr. Mornay very liberally supplied me for experiment, though it necessarily bears marks of the hammer by which it has been detached, presents also other surfaces, not only indicating that its texture is crystalline, but showing also the forms in which it is disposed to break, to be those of the regular octohedron and tetrahedron, or rhomboid, consisting of these forms combined.

In my own specimen, the crystalline surfaces appear to have been the result of a process of oxidation, which has penetrated the mass to a considerable depth in the direction of its laminæ; but in the specimen which is in the possession of the Geological Society, the brilliant surfaces that have been occasioned by forcible separation from the original mass exhibit also the same

                                  configurations

configurations as are usual in the fracture of octohedral crystals, and are found in many simple native metals.

The magnetic qualities of the fragments, fortunately, enable us to appreciate rightly those of the entire mass from which they have been detached ; for though the mass, when tried upon the spot by Mr. Mornay, gave indications of having distinct N. and S. poles, it is pretty clear that these were only so by induction, in consequence of position with respect to the magnetic meridian. For though the fragments are not in the least attractive as magnets, and have in themselves no polarity, they are precisely like any other pieces of the best soft iron, and assume polarity instantly, according to the position in which they are held with respect to the magnetic axis of the earth. When a long fragment is held in a vertical position, its lower extremity being then within 20° of the dip of the N. magnetic pole, becomes N and repels the N. pole of a magnetic needle suspended horizontally. But this power is instantly reversed by being suddenly inverted. So that the apparent contradiction between the observed polarity of the mass, and the seeming want of it in the fragments, is thus completely removed.

Although Mr. Mornay reasonably expected that this iron would not differ from the many others now on record that have been found in various parts of the world, and from his experiments was led to infer the presence of nickel, it appeared desirable to ascertain this point with more precision than he had been enabled to do, and to determine also in what proportion this peculiar ingredient of meteoric bodies might be found to prevail.

I believe the means which I am accustomed to employ for detecting the *presence* of nickel in native iron to be new, and may deserve to be described, on account of the very small quantity of the iron required for this mode of examination.

Having filed from my specimen as much as I judged sufficient for my purpose, (which need not exceed $\frac{1}{100}$ of a grain,) I dissolved it in a drop of nitric acid, and then evaporated the solution to dryness. A drop or two of pure ammonia was then added to the dried residuum, and gently warmed upon it in order to dissolve any nickel that might be present. The transparent part of the fluid was then led by the end of a rod of glass to a small distance from the remaining oxide of iron, and the addition of triple prussiate of potash immediately detected the presence of nickel by the appearance of a milky cloud, which was not discernible by the same means from a similar quantity of common wrought iron tried at the same time.

For the determination of the *quantity* of nickel I employed a
different

different method. A piece of the iron weighing fifty grains having been dissolved in nitro-muriatic acid, the solution was evaporated to dryness. Ammonia was then added, and the solution again evaporated to dryness, in order that the oxide of iron might be rendered more dense, and more easily separated from the soluble portion. A fresh addition of ammonia then readily dissolved the nickel, and the solution after filtration appeared of a deep-blue colour.

A small quantity of sulphuric acid having then been added, the whole was again evaporated not merely to dryness, but with sufficient heat to expel the excess of ammonia, muriate of ammonia, and sulphate of ammonia. The remainder was sulphate of nickel, which was then redissolved in water, and after being suffered to crystallize, weighed 8·6 grains. Having found by experiment previously made for that purpose, that ten grains of nickel give 44 grains of sulphate of nickel, I infer that 8·6 of the sulphate correspond to 1·95 of metallic nickel, which is nearly 4 per cent. of the quantity of native iron taken for experiment.

By an analysis conducted in a similar manner on 23 grains of the scaly flakes of oxide brought home by Mr. Mornay, from the spot where the mass was found, I obtained 3·1 grains of sulphate of nickel, which correspond to 7·05 nickel, amounting to no more than 3·06 per cent. of the oxidated crust taken for analysis. But, if we consider the weight which 100 parts of the metallic alloy would acquire by oxidation, we shall find the two experiments correspond with a degree of accuracy that may occasion more reliance to be placed on these experiments than they really deserve.

96 parts of iron in the state of black oxide will be combined with 28·3 oxygen
and   4   nickel will take
about   1·1 oxygen,

so that   129·4 of the crust will contain only four parts of metallic nickel, and 100 ditto will contain 3·1, which scarcely exceeds the quantity actually found by trial.

From the presence of nickel in this mass we cannot but regard it as having the same meteoric origin with the various other specimens that have before been found ; and although in the spot whence it had been first removed, Mr. Mornay discovered a bed of matter from which it appears, by analysis, that similar iron might be formed by art, it seems by far more probable, that an opposite change has really taken place, and that the whole of this supposed ore is the result of progressive oxidation,

dation, during a series of years of which we have no other evidence, and affords the sole ground on which a conjecture could be formed of the very remote period at which this problematic body has fallen upon the earth.

LXXXV. *Remarks and Suggestions, for further improving and applying to Use, the Government Trigonometrical Survey of Great Britain.—By* Mr. JOHN FAREY *Sen.*

To Mr. Tilloch.

SIR, — I AM much pleased to observe in a contemporaneous Journal, that the *Trigonometrical Survey* of Britain, which has so long been in progress, is beginning more generally to attract the attention of ingenious Men, towards giving to its results, every possible degree of perfection and usefulness, of which they are susceptible.   In one of the communications alluded to, dated October 1816, I read as follows ; viz. "There is also an eminent *Geologist*, Dr. MacCulloch, connected with the work: and at this moment, I believe, going from station to station with Col. Mudge and Capt. Colby, or with which ever of those Gentlemen may be now in Scotland."

It would give me very sincere pleasure to find this latter announcement to have been correct, but of which I can have little expectation, from having, when on a survey of the environs of Edinburgh in the last autumn, been so fortunate, as to have met Capt. Colby, at his station on the Pentland Hills nearest to Edinburgh ; when, in a conversation principally turning on the internal structure of the surrounding District, between a Gentleman of the vicinity, whom I met in the Captain's Tent, and myself, I heard from him or Capt. C. no hint, of such a valuable addition having been made to the Trigonometrical Establishment.

I the more lament this circumstance, from having always desired, and made frequent attempts at recommending the making, of a minute *Mineral Survey* and Map of the Environs of each Station, since I became acquainted in 1801, with the new Principles of *Mineral Surveying*, and with the progress then made in applying them, in extensive practice, by *Mr. William Smith,* for acquiring satisfactory and practically useful knowledge of the *internal structure* of a District, from the accurate investigation and consideration of *its surface;* an art, for which posterity will do Mr. S. ample justice, however long *interested*, Lecturing, Bookmaking and Map-making and publishing parties amongst us may succeed, in suppressing his name and achievements, and in unjustly
appro-

appropriating his labours to themselves, some of them:—but I must restrain for the present my feelings on this unpleasant subject, and with the hope of drawing the attention of your scientific Correspondents to the subject of the National Trigonometrical Survey, I will beg to transcribe a Letter, which in the last Spring I addressed to a public Man, whose useful labours have laid our country under lasting obligations to him: viz.

"*March* 26, 1816.

" Sir,—I am very greatly obliged by your letter of yesterday, and in consequence of the information (new to me), as to the intended experiments on a *Pendulum,* at the principal *Trigonometrical Stations,* beg permission, without delay, to offer to the consideration of your very active and superior mind, a few suggestions connected with this subject.

" *First,* I would beg to recommend, that the *London* pendulum observation should be made, on the floor of St. Paul's Cathedral, or of St. Faith's church under it, exactly *under the Cross* above the dome, which is a most popular and well settled point in the Trig. Survey, and it will be very desirable, that similar observations be made, as near as possible to (E or W of), the Transit Instrument in Greenwich Observatory; and that progressively, these pendulum observations should be made, at every, or at *many,* and those greatly distributed, Trig. Stations.

" *Second,* That by the accurate and ready means which the *Canals* now furnish, and *levellings* for such, which have been accurately made and recorded, when collected, examined and compared, and by other cross levellings from these Canals to the Sea (none of which would now require to be very long), to ascertain *the Elevation of each Trig. Station,* much more accurately and satisfactorily than has yet been done.

" *Third,* I am extremely desirous, for the interest of Science and the honour of our Age, that every possible precision should be given to our Trig. Survey, by employing (at the same time with the pendulum) the capital *Zenith Sector,* which the nation has purchased, and has long and often, I believe, lain *unemployed* in the Tower, in settling the *Latitudes,* from actual observations; so often and carefully repeated, on the Stars, at each Trig. Station (and if confirmed by Pole-star observations the better), as to leave *no remaining doubts,* on this very important head.

" Doubtless, sir, you are acquainted with the able calculations and statements of *doubts* hereon, in the Phil. Trans. by Don Rodrigue, a few years ago; and perhaps also, with the virulent and improper attack on the Roy. for admitting the Don's paper, and the refusals given, to repeat observations, or otherwise, by a proper defence, *to remove these doubts,* by a person,

person, too immediately dependent on Colonel M. as head of the Woolwich Academy, to have been by any means decent or proper, in my estimation, or that of many very competent persons, with whom I have conversed on the subject. ·

" From having, when in the late Duke of Bedford's employ, paid considerable attention to the principles, practice and progress of the Trig. Survey, in and around Bedfordshire, and having made a very great number of careful observations, with each of the very admirable Theodolites they use, and recorded, and applied these in a multitude of minutely accurate calculations of *intersecting* Triangles, and having thus obtained the most ample and satisfactory *checks*, on the results as to *Angles* (when often enough observed) and lengths of calculated *Lines*, I am able to concur most fully with Don R. who has in another mode gone over the whole calculations anew, that the *net-work* of lines spread over the surface of our Island by this Survey (including the measured bases) are *of indisputable accuracy;* but no persons, except some in the Woolwich Acad., who can be deemed competent, will I think be found to say, that *the other important data*, for turning this grand Survey to the physical or philosophical uses of which it is susceptible, have yet been determined, within *any degree of corresponding* or sufficient *accuracy*, to these *mere Lines*, every three of them in different and almost *unknown planes*, strictly speaking.

" That is, as already (and secondly) observed, there is wanting, the *actual heights* of each Station, with reference to *a curved surface of equilibrium* (as yet, of *unknown form*, exactly) formed by the *Sea*, around our Island, and to other nearly similar and parallel higher curved surfaces, inland, which are followed by the several *Canal-ponds* (with some connecting *levellings*, which are wanting), in a connected net-work, over the Island's surface.

" *Fourth,* The ascertaining, by *very numerous repetitions*, under proper and fully recorded circumstances, of heat, pressure, moisture, clouds, &c. the *apparent angular elevation or depression*, of each Station, with every other visible from it; with *much larger and better Instruments*, than have yet been used for *this* purpose ; two of which Instruments should, as often as possible, be *simultaneously* used in observations, at each end of a line between Stations. Calculations from these elevations and depressions, would by comparison with the actual heights, furnish *data*, now exceedingly wanted by Astronomers and Trigonometrical Surveyors, for determining *the Refractions at very low elevations*, some even below the horizontal line (see Phil. Mag. vol. xlvii. p. 2▮▮ and other more considerable elevations, between Church-Tow▮▮, &c. in *valleys*, near Canals (already
well

·well settled in the net-work of Lines) and the Stations on surrounding *Hills,* might be selected, for making this series of *observed data,* as to refraction, join to and blend with the lower ones already obtained by Astronomers.

· " *Fifth,* Very carefully ascertaining *the horizontal bearings,* or Azimuths, of the lines of Triangles, with *the Meridian,* at *every Station;* which yet has been done at a few only, and at fewer still, in a quite satisfactory manner.

" *Sixth,* The *difference of time,* or longitude, to be determined, by often repeated signal-lights, made and observed at the opposite ends of all such long lines of the Triangles, as have nearly an E and W direction. For which last purpose, if two exactly similar apparatus were provided, and simultaneously used, in the *Pendulum* observations, mentioned in your last Letter, the time or trouble of the same, might not be materially increased, by combining these very important longitude observations.

" *Seventh,* That the *Strata,* for a circle of three or four Miles around each station, should be carefully surveyed and mapped, in connection with Mr. Smith's Map of the Strata, which may then sufficiently supply information, as to most of the intermediate parts of the Island's surface.

" *Eighth,* That the direction and degree of the general *dip of the Strata,* at each station, should be carefully ascertained, and a vertical *Section* in that direction, made through the station, of three or four Miles long on each side of the station, either equally so or otherwise, and other cross Sections, in any case where the Station proved to be on, or near to a ridged or curved plane of strata (as is not very uncommon); thus showing the actual thicknesses and forms, and describing the positions of the several strata, as deep as could, without any material expense of sinking on the bassets, be ascertained.

" *Ninth,* That experiments on *the specific gravities* of Specimens of the Stones or Earths, &c. forming each distinct stratum, shown in the Maps and Sections, collected at proper distances below the surface, at several points in the course of each stratum, should be made, on or near to the spot, before the drying or partial decomposing of such Specimens; and that ample and numerous duplicates of all such Specimens, should be labelled and preserved, and deposited in the National Collection (by *Mr. Smith* and others) for illustrating the *British Strata,* now forming in the British Museum; together with copies of the Maps and Sections, having marked on them the exact site of each of such Specimens.

" No progress whatever has yet been made by the Trig. Surveyors,

veyors, I believe (although its propriety was strongly urged, and the means shown for accomplishing it, 14 or more years ago), in collecting the *data* mentioned (Seventh, Eighth and Ninth), although so very important; and such as might, I think, prove satisfactory, towards *calculating the direction of the plumb-line, as affected by the local attractions of the Strata, near each Station ;* and notwithstanding the same is also so important, towards settling the lengths of degrees of the Meridian and of Longitude; and which, thus determined, might secure to this Nation, not only the honour *of first efficiently investigating and mapping the surface of the Strata,* in any Country (by Mr. Smith and his pupils) but also of applying this to scientific use.

 " Before I conclude : permit me to express a hope, at the same time that our *Yard* and the French *Metre* are compared, that a not less useful (and corroborative) comparison may also be made, of our best ascertained or adopted standard *Pound,* avoirdupoise, and the *Gramme* of France.

<div align="right">" I am, &c."</div>

If my leisure would have admitted, of now reconsidering and rewriting my sentiments on the above subjects, this communication might have been rendered more worthy of your pages; as it is, if you are pleased to insert it, and it succeeds in drawing attention to the subjects I have touched on, my ends will be answered: and I am,

<div align="center">Sir, yours, &c.</div>

37, Howland-street, Fitzroy-square.   JOHN FAREY Sen.
      Dec 6, 1816.

LXXXVI. *Some Particulars connected with the late Earth-quake in Scotland. By Mr.* GAVIN INGLIS.

<div align="center">*To Mr. Tilloch.*</div>

SIR, — IN addition to the facts already published in your valuable Magazine, respecting the late earthquake, I send you the following observations, which, as every thing connected with such a phænomenon is important, may prove interesting to many of your readers.

<div align="center">I am, &c.</div>

Strathendry Bleachfield, Kirkaldy,   GAVIN INGLIS.
     Nov. 29, 1816.

The evening of Tuesday the 13th of August was distinguished by no particular appearance in this part of Scotland (Fifeshire) ; but before six o'clock on Wednesday morning, when my work-
<div align="right">people</div>

people were coming to their labour, they *all* expressed a considerable degree of astonishment at the extraordinary number of every species of swallow, that are common in this quarter. The number was far beyond any thing ever seen before, except when they assemble before their departure for the season.

The swallows continued to fly about the field for some days, and then dispersed. They had abandoned their young to perish. In many nests in the neighbourhood, and about the field, the whole young were found dead; also a considerable number of the old ones, six of which I opened, and not the smallest vestige of flies or any other thing was found within them. They had died of want, the number of flies had been so much diminished from the excessive rains preceding that date.

But to the earthquake.—It had been distinctly felt in Kirkaldy and Leslie, but nowhere nearer, nor round Loch Leven, that I can learn. But on Thursday morning about nine o'clock an old man, George Braid, who has lived in this neighbourhood, and been working as bleacher, waulker, &c. upon the banks of Leven, all his life; had just come to the field to waulk some plaidings, and had gone to let down the bye sluice. His attention seemed quite arrested by some appearance in the stream. Standing at a little distance, I called to him, " What's the matter, George ?" " Come and see," was his reply. I went and observed the water perfectly *thickened* with moss, clay, sand, and every description of mud, to a most extraordinary degree. This was the more remarkable as the women had just finished washing a parcel of yarns, when the stream was clean and clear as usual. I observed to the old man,—that the mud and dirt must be the effect of the millers cleaning out Arnott mill dam, and would soon go by. " *No*," said he, " that is *not* Arnott mill dam, nor will it go by for these *eight-and-forty hours* at least."

Seeing his consternation, and the manner in which he spoke, I asked, What made him think so ? Did he, who had lived so long on the banks of the Leven, recollect having ever seen a similar appearance before? " *Never*," said he, " but *once*; and that was when an earthquake happened at Comrie : and be assured there has an earthquake happened somewhere, be where it may."

Nothing of an earthquake had been felt here, nor had the slightest knowledge of any such thing reached this place at the time of this conversation. Struck with the old man's remarks, and his manner of making them, I went up the river to the junction of Arnott mill dam, and found the dam clear as a fountain; but the river under Auchmoor bridge dirty and foul beyond description, and as far up the Leven as the eye could reach.

From

From the top of the bridge the appearance of the water indi‑
cated the same state of filth. Curiosity prompted a further
survey. The stream was followed to the lake, where the whole
mass of waters was found dirtier, if possible, than the stream
that flowed from it. From pretty accurate experiments, the time
necessary for the water just emptying itself from the lake, to
reach Auchmoor bridge, has been ascertained; and upon that
data we calculated, by the time the mud reached the field, at
what time the shock must have been felt in the lake, and marked
it to have taken place some time between eleven and twelve on
Tuesday night.

Judge then our surprise, when the old man's remarks were
so fully confirmed by accounts from the north in so short a pe‑
riod after.

The agitation in the lake must have been dreadful, had it been
day-light to render it visible. The quantity of mud and sand
thrown up from the depth of 80 to 100 feet must have been
very extraordinary, when it continued so long dirty, though it had
the lake itself and the slow windings of a number of miles through
a flat country to subside in. The water continued to run dirty
for two days; so much so, that we could neither work nor put
our yarns through any operation during that period.

LXXXVII. *Remarks on the Article " Strength of Materials,"*
published in Dr. Rees's New Cyclopædia, vol. xxxiv. *part* i.
By A CORRESPONDENT.

THE author of the article " Strength of Materials," after stating
that we have two or three theories by different authors, proceeds
to say, that " it unfortunately happens that we owe all these
theories to men who have not themselves made any experiments."
So far, perhaps, as respects Galileo, Leibnitz, James Bernoulli,
Euler, and Lagrange, the authors particularly alluded to, it is
correct; but it is to Marriotte, whom the author has classed
with the experimental writers, that we are indebted for the first
outline of the true theory; and the whole of his experiments
were made with a view to illustrate and confirm the premises he
had assumed. Marriotte's mathematical investigation only ap‑
plies to rectangular prisms; therefore, his experiments, being
made with cylinders, do not agree correctly with his theory.
Marriotte's theory was published in his *Traité du Mouvement
des Eaux,* sect. v. disc. ii.*

Marriotte and Leibnitz investigated the strength in a hori‑

* See Desaguliers's Translation, p. 237. London 1718.

zontal position; Bernoulli, Euler and Lagrange, the strength in a vertical position: in the former the depth is the variable quantity, in the latter the length and depth are both of them variable: hence the theory of Euler differs from that of Galileo or Leibnitz only in the position of the beam.

Leibnitz, Belidor*, Emerson†, and other writers, have followed only part of Marriotte's theory, admitting the extension to be as the force, but supposing the axis of fracture to be at the lower side of the beam. Leibnitz and Emerson have on these principles attempted to determine the strength of triangular prisms, &c. and have arrived at the most absurd conclusions.

The author treats the subject under three separate heads, and begins with " the direct cohesion of bodies." The strength of bodies to resist a strain in direction of their length is stated to be as their areas; but, allowing the material to be uniform, this can only be the case under a certain limitation, which is not even hinted at in the present article, though it is the cause of the irregularity of Buffon's experiments on the strength of iron. As every material will extend in some degree before fracture takes place, there is a point in the section of fracture through which the direction of the weight must pass; otherwise, the section will not be equally strained, and consequently the strength will not be as the area.

, To determine the centre of resistance, and to find the law which the strength would follow, when the direction of the weight does not pass through it, would be a curious as well as an useful subject of mathematical inquiry.

A want of attention to the direction of the weight is the most likely cause of the strength in certain experiments not being as the area (see Phil. Mag. vol. x. p. 51). As to the different results obtained by different experimentalists,—the various qualities of the same kind of material, the duration of the experiments, and other physical circumstances considered,—they agree as well as could be expected; the highest numbers are most likely to be correct, provided that sufficient time has been allowed for the experiments: but the time, in these experiments, is seldom if ever specified. The next division is " on the resistance of bodies when pressed longitudinally," &c., which certainly should have followed that on the transverse strength, not only on account of its being more difficult, but also because it is only a modification of the transverse strain in which the deflexion becomes the leverage. The author passes over this division of his subject without even attempting a theoretical investigation ; indeed the " intricate" methods pursued by Lagrange and other continental wri-

* *Science des Ingénieurs,* liv. iv. chap. 2. *Arch. Hydralique,* tome iii.
† Mechanics, props. 77 and 78. 4to, 1773.

ters

ters are not calculated to encourage an English author to venture on the subject.

Were we in possession of a correct and simple theory,—and the thing is not impossible, though Euler and Lagrange have failed in their attempts,—it might be applied to practice without any other experimental knowledge than that to be derived from experiments made on beams or bars laid horizontally. Nevertheless, it would have been interesting to have had the tables of Girard in English weights and measures, which certainly ought to have been done, as in its present state it is nearly useless to the English reader. The manner of giving Buffon's experiments is still less excusable, because they would have been better in their original form than as they are.

The experiments on the strength of stones, which are taken from the *Encyclo. Britannica,* were published by M. Gauthey, in 1774, in Rozier's *Journal de Physique,* and were made in consequence of a Memoir which M. Patte published in 1770, in which he expressed his doubts respecting the stability of the pillars which support the dome of the French Pantheon. The celebrated Soufflot, having heard of these experiments, constructed a machine similar to that used by Gauthey, and Perronet made a third; but it was perceived, in the course of their experiments, that the friction and change of position of the lever were so considerable as to influence the results. To remedy these defects a new machine was invented by M. Rondelet, with which numerous experiments have been made. These are published in his *Traité de l'Art de Batir,* tome iii. p. 81, together with a description of the machine. It is to be regretted that the direct cohesion of the stones crushed by this machine has not been ascertained.—But to return to the author of the article: He says, " We are aware that in different materials a different law may be observed between the strength of direct cohesion and the resistance of the same body to a transverse strain." It would be desirable to have some further particulars if this really be the fact, but I should rather suspect the writer to be mistaken.

The third division is " on the transverse strain and strength of beams," &c. The theories of Galileo and Leibnitz present such erroneous results when applied to any other than rectangular beams, that it is surprising, where a want of space is more than once complained of, so much should be occupied in discussing them, to the exclusion of other valuable matter;—for instance, the resistance to twisting, and also what Dr. Young calls resilience and detrusion, might have been introduced. In the account of the experiments of Du Hamel there is a mistake, which also oc-

curs in Dr. Robison's account of them. There were only two
bars cut one-third through, and only two cut half through ; see
Du Hamel's *Transport du Bois*, p. 421. Paris, 1767.

The author prepares us for his own experiments and calcula-
tions by observing, " that Galileo is much nearer, if not exactly
conformable to the actual operation." But without giving a rea-
son for this opinion, he gives a rule founded on experiments
which are not detailed, and it is probable the rule will require
to be varied to suit different kinds of materials, though the only
variables that ought to have place in it, viz. the direct cohesion
and extensibility, are both included. The author must be aware
that a rule of this kind, however correctly it may agree with
one set of experiments, will not agree with another; therefore,
such a rule must be empirical. Besides, if it be necessary to
resort to a rule of this kind, it ought to be simple; which cannot
be said of the present one. Several rules have been given at
different times from the experiments of Buffon, and a very easy
one is given in Brewster's Encyclopædia, art. *Carpentry*, p. 508;
but were these rules correct for timber newly cut, they would be
incorrect for dry timber. To attempt to include accidental
qualities in a general rule is absurd, yet this must always be the
case with rules founded on experiment.

The author remarks, that the deflexion " is not a necessary
datum in estimating the strength of timber for any practical
purposes of building." Had he said, it is the only one that is really
useful, he would have been much nearer the truth. See Dr.
Young's Nat. Philosophy, vol. i. p. 138; or his paper on Sep-
ping's Method of Ship-building, Phil. Trans.; or Buchanan's
Essay on the Shafts of Mills, p. 73.

If we had a correct theory, as far as the direct cohesion and
extensibility of the material are concerned, and four or five ex-
periments, under different circumstances, on each kind of ma-
terial, the material being accurately described, its specific gravity
and extensibility correctly ascertained, and the manner of making
the experiment accurately detailed,—then we should have a series
of results under various circumstances, which would enable the
mechanic to apply the theory to practice, with such limitations
as a comparison of his material with that of the experiment
would readily point out to him.

As long as the practice of giving mean results without de-
tailing the experiments shall continue, so long may we expect
to remain ignorant of the nature of the resistance of solids. In-
stead of following the methods of Prony or Laplace, the example
of the chemical writers ought to be followed. This will lead us
insensibly to perfection, while that is but a specious covering for
ignorance.   I do

I do not mean to say, there are no cases where the theory of Laplace can be applied with advantage: but they are very rare; and it ought only to be resorted to when there is not the means of obtaining correct information.

LXXXVIII. *On the Ventilation of Coal Mines.*

Newcastle, Oct. 5, 1816.

W<small>E</small> whose names are undersigned, being persons who composed the meeting held at the Assembly-rooms, Newcastle, on the 9th of September 1815, observing that Mr. Ryan has affixed the resolution of that meeting, with our signatures, to his " Letter to the late Secretary of the Society of Arts, on his Method of ventilating Coal-mines," feel ourselves called upon to declare, that no part of that method was then explained to us, but simply an application of the inverted siphon to the clearing away of collections of carburetted hydrogen gas, which may have accumulated in cavities in the higher levels of a coal-mine; which he illustrated by the common experiment of a bended glass tube immersed in two fluids of different densities.

In consequence, however, of this general explanation of Mr. Ryan's *principle,*—the particular application of which, at the same time, he expressly declined communicating,—we thought it right to give our testimony of general approbation of it, " as consistent with the principles of philosophy;" and, in consequence of his assurances of its successful application in certain coal-mines in Staffordshire, " recommended the propriety of its adoption to be taken into consideration by persons interested in the coal-mines."

On the merit or demerit of the plan now pointed out by Mr. Ryan, we do not feel ourselves called to give an opinion. Our only object is to state, that no communication of it was made to us at the meeting above alluded to.

R. W. Grey.	John Hodgson.
J. H. Bigge.	Christ. Benson.
William Turner.	Wm. Clark.
Nat. John Winch.	Wm. Armstrong.
John Carr.	Rob. Wm. Brandling.

The preceding document would have appeared in the last number of the Philosophical Magazine, had we not been disappointed of the wood-cut which now accompanies a few remarks, which we think the subject calls for. We examined with some care Mr. Ryan's plan for ventilating mines : we think we un-

E e 3 derstand

derstand it ; and being persuaded that it promises many important benefits to miners, we were not backward to express our opinion. The gentlemen who sent us the above would, we are confident, be sorry, should any statement of theirs retard the introduction of an improvement calculated to add to the security of miners ; and as we think that Mr. Ryan's plan of ventilation may actually be compared to the operation of an inverted siphon, it is possible, if this can be made appear, that the objection which these gentlemen have taken, to the use made of the certificate alluded to, will be thought, by themselves, to be groundless.
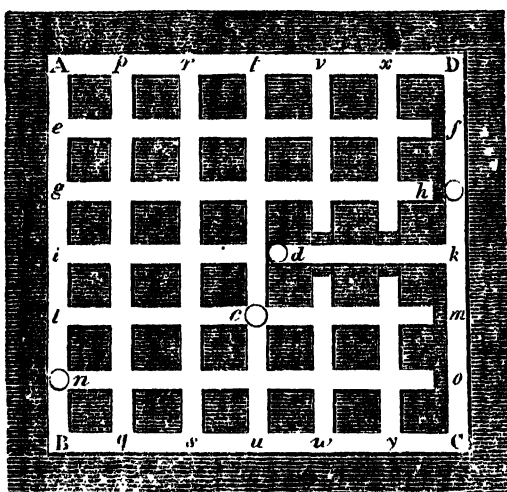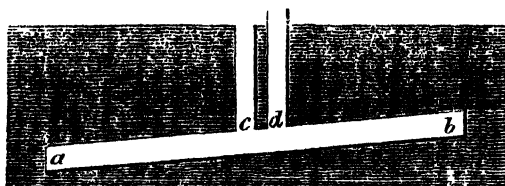
Let the inclined path or working *a b* represent a section of a mine; *c* the down shaft by which the atmospheric air is to descend; and *d* the up shaft by which the inflammable gas is to be discharged. When a current is once established, either by the greater height of the discharging shaft*, or by the application of heat to some part of it, the natural tendency of the de-

* When necessary, Mr. Ryan increases the height of the up shaft by means of mason or brick-work.

scending atmosphere, owing to its greater gravity, will be, to take the lowest station, descending along the bottom of the path or working from *c* to *a*, while at the same time the carburetted hydrogen by its levity being sent to the roof will proceed onward to *b* till the space from *b* to *d* be filled with it, when it will ascend the shaft *d*—the heavier aëriform fluid filling and occupying the lower station.

Let ABCD represent a ground plan; *e f, g h, i k, l m, n o*, inclined workings; and *p q, r s, t u, v w. x y*, the cross workings. Let the down shaft and the up shaft be supposed placed in the most favourable circumstances—say, the former at *n* and the latter at *h*, and the mine will ventilate itself. But owing to some cause, we shall not inquire what, these shafts shall be supposed placed in less favourable circumstances—say, the down shaft at *e* and the up shaft at *d*. How is this mine to be ventilated? By forcing the descending atmospheric air to go down one inclined working and up the next, then down the third and up the fourth, &c. traversing the whole workings (the cross workings being stopped up where necessary) till, after travelling 25 or 30 miles, the atmospheric air blended with and diluting the inflammable air, both be conducted to and discharged by the up shaft? No, says Mr. Ryan : take advantage of the law of gravity, the heavier air will take the bottom, the lighter the roof; and by proper management the lighter may be directed to the up shaft in an unceasing current, with a velocity proportioned to the quantity liberated from the workings, the pressure of the air in the down shaft, and the artificial means (the application of heat, &c.) which in any case may be resorted to. How does he effect this? Let a gallery ABCD be excavated all round the workings. Let the cross workings immediately on each side of the inclined course which descends from *k* to *d* be left uncut, or, what is the same thing, stop them up so effectually that no air of any kind can reach the up shaft *d*, but what must come from the upper part DC of the air gallery ABCD. In this case though the gas will have to descend from *k* to *d*, before it can reach the shaft *d*; the current being once established it will meet with no impediment, the lighter carburetted hydrogen being pressed upward by the heavier atmospheric air, descending the other shaft, and occupying the lower station. Let the course of the inflammable gas be ever so tortuous, like water discharged downward by a siphon (however crooked) it will find its place of discharge upward at the orifice of the longer limb [shaft].

Such we conceive to be a brief outline of Mr. Ryan's system. In such a short article we cannot pretend to have done it perfect justice ; much less can we enter into details respecting the vents and openings, which in some cases may be rendered in-

dispensable, owing to faults and breaks in the strata. We have only aimed at showing that his system is " consistent with the principles of philosophy," and that it is " simply an application of the inverted siphon," discharging *upward* a fluid lighter than the one employed to displace it.

We are confident that the gentlemen who signed the above document will do justice to the motives which have induced us to add these remarks; for, having the honour to know some of them personally, we know that none can more earnestly desire to see any practicable system introduced which can contribute to the health or safety of the miners. Mr. Ryan, being almost self-taught, is by no means perspicuous in conveying his ideas; and without bestowing much personal trouble in attending his examinations at the Society of Arts, besides several other meetings with him, we should not, simple as his plan is, have easily comprehended it. We are therefore not surprised that others, without the same means, should find it difficult.—But this very circumstance has made us the more anxious to render it intelligible, in as few words as possible; for, to speak truth, Mr. R. possesses in a wonderful degree the art of *mysterizing* what is in itself very simple; but this should not be allowed to operate to prevent the public from reaping the benefits which his system may be calculated to confer.

We have only to add, that the gentleman who sent us the above document informed us, that it would also have received the signature of Mr. Dixon Brown, had he not been from home at the time; his sentiments on the subject being known to accord with those who signed the paper.

LXXXIX. *On the late Solar Eclipse.*

To Mr. Tilloch.

Sir, — In the last number of your valuable Magazine, is a communication from Mr. Groombridge, respecting the late eclipse of the sun. In another Journal (the Monthly Magazine) is a communication from Mr. Evans, relative to the same subject. On comparing the times, as observed by these two gentlemen, it will be seen that there is a difference of *fourteen seconds* in the time of the commencement, and of *twenty-eight seconds* in the time of the ending of this eclipse. As these two observers live within a mile or two of each other, there cannot be any great difference in the absolute time at the two places: and as they have thought proper to publish the result of their observations,

I think

I think they owe it to the public to reconcile this singular difference between them.

The same remark will apply to two other observers in the town of Ipswich, viz. Mr. Capel Lofft, and Mr. Acton, as inserted in the same Magazine to which I have just referred. Here we find a difference of no less than *one minute and a quarter* in the time of the ending of this eclipse, although they both agree as to the time of the commencement. This difference appears to be there attributed to the superior power of one of the telescopes: a very improbable and a very absurd solution of the difficulty. For, independent of the impossibility of any telescope (however powerful) making so wonderful a difference in the ending of this eclipse, it ought to have caused a similar difference in the time of the commencement. Indeed we sometimes find that there does exist a slight difference in the observed time of the commencement of an eclipse, but seldom any difference in the time of the ending of it: because in the former case, one observer may catch the first impression of the moon on the sun's disc before the other; but, in the latter case, they can both follow the moon's course to the very edge of the sun's disc with equal facility and correctness, and thereby tell *exactly* the time at which the separation of the two bodies takes place.

Before I close this letter, I cannot help adverting to Mr. Groombridge's explanation of the error in the Nautical Almanac, which had been noticed in one of your former numbers, relative to the point where the moon makes the first impression on the sun's disc, in this eclipse. He says that the number of degrees is right if applied to the moon's vertex *as it relates to the plane of the equator,* and that it ought to have been so expressed. But surely it cannot be necessary to inform Mr. Groombridge that in *all* cases of this kind, the angle is reckoned from the vertex as referring to the plane of the *horizon* only; and that the very design of noticing it in the Nautical Almanac, was to enable an observer to point his telescope to that part of the sun's disc: which could not be done if the vertex related to the plane of the *ecliptic* or of the *equator.* In fact, Mr. Groombridge cannot produce any Nautical Almanac, ever since the first publication of that work, wherein any other interpretation of the phrase is intended: neither do I think that he can mention any treatise on the science, wherein any other mode is given, for determining the point where the moon first touches the sun's disc, than that which measures the angle from the vertex *as it relates to the plane of the horizon.*

One word more on the subject of this eclipse. Mr. Groombridge observes that it was total in the *northern* parts of Russia,

on or near the meridian. But, if he will take the trouble to calculate the path of the moon's shadow, he will find that the sun was centrally eclipsed on the *meridian* in north latitude 43° 8′ 48″, east longitude 29° 41′ 30″; and that the central path of the moon's shadow did not, at any time during its whole course, enter any part of the Russian territories.

<div align="center">

I am, sir,

Your obedient servant,
</div>

London, Dec. 16, 1816. <div align="right">ASTRONOMICUS.</div>

<div align="center">

XC. *On the late Solar Eclipse.* By E. WALKER, *Esq.*

To Mr. Tilloch.
</div>

DEAR SIR,— THE following observation on the solar eclipse, of November 19, 1816, was taken in latitude 52° 45′ 24″ N. and longitude 1′ 34″,4 in time, east of Greenwich.

The beginning of the eclipse was not observed, but the end was taken, with a forty-six inch achromatic magnifying forty-two times, at .. .. 10ʰ 25′ 14″,5 A.M. per clock.
Clock too fast at 10ʰ 25′ .. 0 3 9,66

End of the eclipse .. 10 22 4,84 m. time.

The air was very clear during the time of observation, and the aperture of the object glass of the telescope being contracted to two inches, the distinctness gained by this means was more than a compensation for the low power I used. And, as I had an observation of the sun's transit over the meridian on the same day, the error of observation of the end of the eclipse does not, probably, amount to more than a second.

<div align="center">

I am, dear sir,

Yours respectfully,
</div>

Lynn, Dec. 10, 1816. <div align="right">EZ. WALKER.</div>

XCI. *Inquiries into the Laws of Dilatation of Solids, Liquids, and elastic Fluids, and on the exact Measurement of Temperatures.* By Messrs. DULONG *and* PETIT. *Read to the Institute* 29th *of May* 1816.

<div align="center">

[Concluded from p. 380.]

Of the Dilatation of Solids at high Temperatures.
</div>

THE knowledge of the dilatation of the metals, particularly of the ductile metals employed in the construction of instruments
<div align="right">and</div>

and machines, is very important in a great number of circumstances. The utility of these determinations has been at all times felt; and they have employed methods more and more exact, in proportion as the want of precision was felt in physical researches. We may cite as most deserving of confidence the results of Smeaton, Ramsden, Lavoisier, and Laplace

In the labours which have been undertaken on this subject, we have principally endeavoured to ascertain the absolute dilatations from 0° to 100°, and this is in fact sufficient for common use. Messrs. Laplace and Lavoisier have besides ascertained, that between the freezing and boiling point the dilatation of the metals was exactly in proportion to the indication of the mercurial thermometer. Borda also made the same comparison, but in an extent of thermometrical scale too inconsiderable to warrant any induction relative to the laws of dilatation. Nevertheless the object of his labour was fulfilled, since he proposed merely to ascertain the variations of length which were produced by the changes of temperature of the air in the instruments which were to be employed in the measurement of the meridian.

The greatest difficulty experienced by all those who have been occupied with inquiries into the dilatation of solids, proceeds from the necessity in which we are of rendering one part of the system absolutely fixed, which implies much complication in the apparatus. When we confine ourselves, as Borda did, to measure the difference of dilatation of two metals, we are merely obliged to render the instruments invariable with respect to each other, a condition which it is much more easy to perform than the first.

As we only seek to compare between each other the laws of dilatation of various bodies, it appeared to us that we should equally well attain this end, by substituting the difference of the dilatations for the absolute dilatations.

It is chiefly from this consideration that we determined to employ the following apparatus :

This apparatus is composed of two rods, one of platina, the other of red copper, twelve decimetres long, twenty-five millimetres broad, and four millimetres thick: they are connected in an invariable manner by one of their extremities, by means of an iron *traverse* upon which they are firmly screwed. To the other extremity are adapted brass rods, which rise at first vertically and afterwards bend horizontally. One of these rods has a scale graduated into fifths of a millimetre, and the other is furnished with an index which marks the twentieths of the division of the scales, which admits of our appreciating the centiemes

tiemes of the millimetre or the 120,000dth parts of the length of the measuring rods.

These rods rest on four copper rollers fastened to a bar of iron. The whole machinery is placed in a vessel of red copper fourteen decimetres long, fifteen centimetres deep, and ten centimetres broad. We made use of fixed oil in these new experiments as in those which were made upon the gases, and we had recourse to the same means to render the temperature stationary during a time sufficient to permit the measuring rods to acquire an equilibrium of temperature with the liquid. There were also two systems of metallic plates placed on each side of the measuring rods, and which we might put in motion in the liquid mass, so as to mix the different strata, and establish everywhere a uniform temperature without any risk of the derangement of the rods. Finally, the copper vessel was furnished with a lid having four sockets, in which were thermometers which served to indicate the differences of temperature which were expected to take place in the various parts of the vessel. A thermometer placed horizontally between the bars indicated the true temperature of the liquid.

That we may avoid prolixity, we shall omit several trifling details, which concurred however to the exactitude of our observations. It is easy to see from the known dilatations of the platina and copper, that a change of one degree in temperature ought to vary the relative position of the index by about a hundredth of a millimetre. Now it is impossible to be deceived with a part of the index, however little a person may be versed in reading off the divisions. This precision appeared to us sufficient in the kind of researches which occupy us. Besides, in order to give more sensibility to this instrument, its dimensions must be augmented, and then the difficulty of establishing a uniform temperature throughout the whole extent of the measuring rods would necessarily throw more uncertainty on the true dilatations.

As a point of departure, we have taken the state of the measuring rods in an oil-bath, which remained several days in a room the temperature of which did not sensibly vary. This process appeared to us to be preferable to the employment of ice, which does not present a temperature really fixed, unless we can agitate it continually, particularly when the surrounding air is from 15° to 20° above zero.

The oil-bath was afterwards heated to the temperature of 100 degrees and upwards, with the same precautions as in the foregoing experiments. The heat of the liquid mass and that of the furnace being here very considerable, the maximum of temperature

was

was kept up for ten or twelve minutes, which was sufficient for the rods being put in equilibrium of temperature with the oil, if we consider particularly that the agitation of the liquid renewed every instant the surfaces in contact. (The proof of this was besides acquired by the invariability of the indications of the index.)

The temperature was afterwards raised to 200, 250, 300°, and the simultaneous observation of the index and of the mercurial thermometer served for comparing the progress of this last instrument with the dilatation of the metals. On taking the difference of dilatation corresponding nearly to the first hundred degrees of increment in the temperature, in order to ascertain the value of the degree in our metallic thermometer, we found that the term of 300° of the mercurial thermometer corresponded nearly with 310° of the metallic thermometer. These experiments, which were repeated several times with results very little different, prove, contrary to the opinion generally received, that the metals follow in their dilatation a progress more rapid than that of the mercurial thermometer. Thus by supposing that we had regulated, according to the method adopted, the thermometer in the air, the mercurial thermometer, and the metallic thermometer, when the first shall mark 300° on its scale, the second will indicate nearly 310°, and the third 320°.

This property of the metals being once well ascertained, we must naturally inquire if the glass, which we are obliged to employ in almost all the experiments, does not present the same phænomenon. We endeavoured to ascertain this by adding a measuring rod of glass to the copper measuring rod of the foregoing apparatus; but a difficulty was then presented, which we do not meet with in the metals; the measuring rods ought to be kept in a position absolutely invariable with respect to each other, and this cannot be done without a screw. Now all the world knows that it is impossible to fasten strongly a thick metallic plate against the glass without breaking it, whatever care be taken to prepare the surfaces which are to come in contact. We employed in the first place, as an intermediate body, a sheet of paper which had been previously brought to a temperature of 300° by taking care to compress it strongly between two metallic plates. In spite of this precaution, the glass rod did not appear fixed with sufficient solidity after the experiment, to remove every suspicion of error. We then substituted for the paper very thin laminæ of fine silver; and for this time the immoveability of the measuring rod and of its prolongation appeared to us complete.

We have made with this system of measuring rods several series of experiments similar to the foregoing, and we found
that

that the excess of the dilatation of the copper over that of the glass remained very nearly in proportion to the indications of the mercurial thermometer to the temperature of 300° and upwards. This result, very different from that which we had obtained with the measurements of copper and platina, as it appeared to us, could only be accounted for by supposing the glass to possess a law of dilatation still more rapid than that of the metals.

Before us M. De Luc thought he perceived in this substance a similar property, although his experiments had not gone beyond 100°. His apparatus was composed of two vertical measuring rods, one of glass and the other of brass, fastened together invariably at their lower extremities. He gave to these measuring rods lengths which are in the inverse ratio to their respective dilatability: the largest, which was the glass one, is kept fastened by its upper part; the highest extremity of the copper rod is entirely free; and it results from the relation of the dimensions of the two measuring rods, that this extremity cannot undergo any change of situation at whatever temperature the measuring rods are exposed, if the dilatabilities of the glass and of the copper vary in proportion. Now De Luc had observed that when he had established the compensation for a certain change of temperature, there was no longer any room for greater or smaller variations.

But we must observe, that the measuring rods were in a vertical situation, and plunged into a vessel full of water, of which the temperature was gradually raised. Although care was taken to shake the liquid, it is probable that the inferior strata were always colder than the upper; and as the copper rod occupied the lower half, while that of glass passed through it in its whole height, we may ascribe the increasing dilatability which the glass has presented, to the copper rod being always found at a lower temperature than the other, and the difference must have increased with the heating. We might besides remove this doubt, by repeating the same observation, the measuring rods being placed horizontally, or rather by reversing the position of the apparatus.

M. De Luc, who does not seem to suppose that his apparatus has the inconvenience we have mentioned, employed no means of verification; so that we might fairly entertain some well founded doubts as to the result of his experiments. With the view of verifying the consequences which we may deduce from the observations of this experimentalist and our own, we have endeavoured to establish this same comparison by a more simple method.

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The march of the mercury in the ordinary thermometer measures the excess of the dilatation of this liquid over that of the substance which serves it for an envelope: when this substance varies, the apparent dilatations of the mercury change; but they will all follow the same law, if the bodies of which the envelopes are formed have themselves a similar law of dilatation. M. Biot has already made a fortunate application of this truth to the determination of the real maximum of density of water, and to the inquiry into the laws of dilatation of various liquids.

It was by resting on the same principle that we endeavoured to compare the dilatations of the glass and iron at various temperatures. In order to know the apparent dilatation of the mercury in the glass, it is sufficient to observe the thermometer itself: in order to measure this same dilatation in the iron, it is sufficient to construct a thermometer the reservoir and tube of which are of iron. We cannot, it is true, estimate directly in this case the increase of volume; but by substituting the measurement of the weights for that of the volumes, the determinations are not less exact, when we operate on a large mass.

The instrument which we made use of consisted of a cylindrical reservoir of hammered iron, very homogeneous, which might contain about 135 grammes of mercury. It is surmounted by a cone taken in the same mass, and terminated by a small hollow steel tube, the interior diameter of which does not exceed a demi-millimetre. We may screw to the upper part of this tube another reservoir of the same metal, the capacity of which is double that of the lower: when the two pieces are joined, the extremity of the tube enters two or three millimetres into the interior of this cylinder. We see by this arrangement that we may fill the reservoir and the tube like a thermometer, by boiling at different times the mercury, so as to avoid all suspicion of the air or humidity having any effect.

When the vessel has assumed the desired temperature, we unscrew the upper reservoir, and the lower will be found perfectly filled with mercury. On submitting it afterwards to a higher temperature, the mercury which issues from the thermometer of iron may be received into the upper reservoir. By weighing precisely the empty vessel, and this same vessel containing the mass of mercury which filled its capacity at the different temperatures observed, we may see by a very simple calculation, if the supposition of a similar law of dilatation in the iron and the glass will make the results of the experiments agree with each other.

We could not expect to find in the interval of the first hundred thermometrical degrees a very sensible variation in the ratio of the dilatations of the two substances which we compared; but

but we made the experiment at the boiling point of water, in order to assure ourselves of the degree of exactness which we might expect from our process. By employing in the calculation the dilatation of iron given by Messrs. Laplace and Lavoisier, we found the dilatation of the mercury $\frac{1}{5411}$ for one degree. We must add an unit to the third cypher significative of the dilatation of the iron which we have employed, in order to make our result coincide exactly with that of the same experimentalists on the expansion of mercury.

Now by exposing the vessel at the temperature of 300° we find that the quantity of mercury which issues from the iron thermometer is far superior to that which ought to issue from it, if the iron and the glass preserved at high temperatures the ratio of expansibility which has been assigned to them below 100°.—This experiment has been repeated several times, with results very little different; so that we may conclude that the dilatation of the glass does not remain constant at all temperatures, and that it increases more rapidly than that of iron.

It is extremely probable that the dilatation on the increment of elastic force of the gases remains constantly proportional to the temperatures. (This is what we shall prove in a subsequent paper.) By admitting this principle, we see that the indications of the mercurial thermometer will be always superior to the real temperatures, and the more so in proportion as we rise in the scale; but experimentalists will doubtless see with astonishment, the slowness with which these differences increase. This does not seem to arise from the dilatations of the mercury remaining nearly proportional to the real increments of temperature, but from the law of dilatation of the glass being combined with that of the mercury, and an almost exact compensation being the result. Besides, we shall be able to verify this completely.

It results also from our researches, that the metallic pyrometers, to which we ascribe a regular march, indicate temperatures much too high, when we suppose, as has always been done hitherto, that the dilatation of the metals remains constantly proportional to the temperatures.

Note at the conclusion of the above Paper by the Editors of the Annales de Chimie et de Physique.

This memoir is printed precisely as it was presented to the Institute in 1815. We then expected to have been able to publish the continuation speedily afterwards.—Particular circumstances having interrupted our labours, and not permiting us to fix the period at which it will be terminated, we thought it best to give to the public the results which have been already obtained.

XCII. *New*

XCII. *New Theorems applicable to the Value of Annuities.*

To Mr. Tilloch.

Sir, — THE following correct and new theorems, for deter-mining in all cases the amounts of annuities certain when in-creasing in the constant ratio of the natural numbers 1.2.3....n, and also of the squares and cubes of the natural numbers, may prove acceptable to many of your readers.

The method of summing of series of this kind is very well known, but in this instance I was led to them by a process es-sentially different. However, my object is not to discuss methods and principles, but to detect error with a view to the consequent establishment of truth ; and as this matter appears not to have been clearly illustrated by any of the writers on series or an-nuities with whom I am acquainted, I am therefore desirous to announce these theorems in your publication. Mr. Baily, in his Doctrine of Annuities, has given the investigations relating to this subject ; but in the several series there made to represent, respectively, the amount of these annuities, a remarkable incon-sistency exists, the consequence of which is, that the formulæ, thence derived, are of no avail in satisfying the objects of our inquiry when the annuities are thus annually increasing ; and further, that they are applicable to this purpose, only on the as-sumption of the annuities being actually decreasing in the order specified above. It is therefore proper that the two assumptions be separately distinguished, and this ambiguity in the formulæ pointed out: otherwise the adoption of it indiscriminately, may not only confuse and mislead, but produce egregious mistakes. At the same time, I cannot but express my surprise that an au-thor, who censures with so much freedom and severity the scien-tific labours of his contemporaries, should have suffered such absurdities to deteriorate what in every other respect is a very useful and popular performance.

I shall now proceed to give the theorems themselves ; in order to which I take X to denote the amount of an annuity increasing according to the order of the natural numbers 1. 2. 3....n; and Y and Z that increasing by the squares and cubes of them.

Now putting x to denote $1+r$ the amount of $1l.$ for a year, and n the number of years ; then these several quantities will be equal to and truly represented by the following different series:

$$X=n+(n-1)x+(n-2)x^3+(n-3)x^3+(n-4)x^4+(n-5)x^5 \; . \; . \; \ldots x^{n-1}$$

$$Y=n^2+(n-1^2)x+(n-2^2)x^2+(n-3^2)x^3+(n-4^2)x^4+(n-5^2)x^5\ldots x^{n-1}$$

$$Z=n^3+(n-1^3)x+(n-2^3)x^2+(n-3^3)x^3+(n-4^3)x^4+(n-5^3)x^5\ldots x^{n-1}$$

• F f And

And the analytical expression for the sum of each series will be respectively :

$$X = \frac{\overline{x-1}.(1+r-1)-}{(x-1)}$$

$$Y = \frac{\overline{x-1}.\overset{x+1}{(1+r-1)}-(2n+1+r.n+1^{2})}{(x-1)^{2}}$$

$$Z = \frac{\overset{6x}{\overline{x-1}}.(1+r-1)+r.1+r-r.\overset{n+1}{^{2}n}+1^{3}+3(r.\overline{n+1}^{2}+2n+2+r.\overline{n+1})-r}{(x-1)^{3}}$$

As an example in each case when the term is five years, and rate of interest 5 per cent., we have for

$$X\ 16,03825625$$
$$Y\ 57,56850625$$
$$Z\ 232,4440064.$$

But which numbers, by the erroneous process of computation given in the work already quoted, would come out successively for this term and rate of interest:

$$X\ 17,11553125$$
$$Y\ 64,03215625$$
$$Z\ 265,19378125.$$

The difference in each case, between the amounts or the excess in error, rapidly augmenting as the rate of interest and term of years increase.

To the formulæ enumerated above, may be added the following :

$$\frac{\overset{x}{\overline{x-1}}.\overset{n}{(1+r+1}+\overset{n-1}{r-2)}-(2n-1)}{(x-1)}$$

And this is a general expression for the amount of an annuity increasing according to the numbers $1.3.5.7...2n-1$.

And if the annuity is supposed increasing in the order of their squares, the general expression for its amount in n years will be

$$\frac{\overset{8x}{\overline{x-1}}\overset{n}{(1+r-1)}+r.1+\overset{n}{r-r}\ (2n+1^{2}-3n}{(x-1)^{2}}$$

I intend at a future opportunity giving the necessary theorems for the present value of annuities under the particular circumstances just stated, and also some other matter relative to the determination of the rate of interest in annuities.

5, Haberdashers Place, Hoxton,　　　　　　JAS. BENJ. BENWELL.
　　　Dec. 10, 1816.

XCIII. *On Safety-lamps, and the Barrier formed by Wire-gauze against the Passage of Flame.* *By* J. Murray, *Esq.*

To Mr. Tilloch.

Sir, — Mr. Langmire, in the last number of The Annals of Philosophy, pronounces the limit 1 have assigned to the barrier against flame—to be inconsistent with the deductions of Sir H. Davy. By a reference to my paper Mr. L. will find that this had a relation to simple unexplosive flame, and was introduced as explanatory of the chasm obtaining between flame and metallic surfaces; and if this gentleman will have the goodness to place a series of wires on a frame, these wires being one-eighth of an inch apart, he will find (as I did) that such an arrangement is effectual in cutting off the communication of flame. If he will please to be referred to the Rev. Mr. Hodson's Narrative in your last number, page 351, he will also discover that Sir H. expressly names *one-eighth;* this I hail as corroborating my inference, and value it the more as it has been for the first time *to me* published.—The safety-lamps are constructed of wire-gauze having 784 apertures in the square inch; and I repeat, that I do not consider any danger is to be apprehended, if by accident the alternate mesh be broken down; for even then, the intervals would be only distanced *one-fourteenth of an inch.*

The *double* cylinder is by no means necessary, and may be injurious where the exterior one is of *copper,* for the following reasons:

1. It is violently acted upon by fire-damp.

2. Dissimilar metals being employed, a Galvanic action may be induced, and *corrosion* take place. And,

3. The sebacic acid of rancid oil acts forcibly on copper.

I will go further than even Sir H. Davy has done, and assert, that, even should *an explosion* occur within the lamp (the gauze being formed of loose pliant materials), and the imperfectly constructed cylinder *burst,* by means of it; even then, no danger would accrue to the miners, for the *extinction would instantly succeed the explosion,* when confined within the cylinder. In one of the collieries which I descended, the under-viewers using a lamp procured from Newcastle, made of fine *brass* wire as soft and pliant almost as common thin writing-paper, the cylinder loosely connected with the socket, *above* the screw, and the seam indifferently joined;—this improper instrument, having nothing to recommend it but a *rude imitation* of that most valuable machine, *burst in his hands* at the *joining* of the *seam;* yet he was perfectly secure, and escaped without the least injury.

The

The seam was stitched together with intervals not less than ⅛th of an inch; and this had served *repeatedly* to explore the mine and ascertain the presence of the fire-damp.   These facts are important to our question, and place it in a new and interesting point of regard.

It is harsh and unbecoming, I deem, sir, to ridicule this happy invention; for myself, I cannot make merry when the question at issue is that of the lives of human beings:—Is it not too severe to blend the name of its discoverer with contemptuous sarcasm?—introduce the language of the admirers of this principle of security with pantomimic farce?—and to turn the praise honestly bestowed on Sir H. Davy, into invectives against him? I should be glad to know who it is that has done *more* for chemical science than this philosopher (I would not depreciate the labours of his contemporaries, or undervalue preceding chemists): What discovery, I ask, has ever imposed a higher claim than this safe-lamp, on our tribute of admiration?   Mr. Langmire *suspects* it is unsafe—I, on the other hand, *know* that it is secure under every possible circumstance.   This language Mr. L. will pronounce bold and assuming;—but it has been proved safe under circumstances which can *never* occur in the mine.—A friend of mine got a small safe-lamp, not more than one *half* the size of those in common use, and where the danger was proportionally enhanced, this,   containing 784 apertures in the square inch, was plunged into a mixture of *oxygen and supercarburetted hydrogen:* the wire-gauze became speedily *red hot;* but it was found even under this severe test *perfectly* safe.   Sir Humphry Davy is blamed, I think unfairly, for the *mere mechanical* part of the structure; and the term "lamp making," and the word "original," are associated in a manner which is unfeeling and undeserved.   The calm and dispassionate suffrages of the gentlemen who voted to Sir H. a piece of plate, for the eminent service rendered to the cause of miners, forcibly repel the unjust attack.   Would it not be an insult to their understanding and good sense, to say that the merits of the discovery had not been fully canvassed, or that they were incapable of appreciating the benefits rendered to humanity by its means? I challenge in the most unequivocal manner Mr. L. to point out a single accident which has occurred where Sir H. Davy's lamp, as constructed by Mr. Newman, of Lisle-street, has been used. This is the most decisive appeal that I can make, or can be made, in its favour,—I think I could guaranty *with my life* its absolute safety.   The *wear* of the materials is a point which none can prevent—mutability is impressed upon all the works of Nature. To defy or controul the operations of natural causes, to endeavour to stem the ravages of time, is a folly which could never

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enter a mind so well fortified by science as that of the emi-
nent philosopher against whom Mr. Langmire has so desperately
wielded the shaft of ridicule.—As to the *priority* of Sir H.
accused by Mr. L. (*indirectly* it may be) of acting on the ori-
ginal invention of another, Mr. Hodson's view of the controversy
will fully confirm: and to that paper I beg that Mr. Langmire
will direct his attention.

None can doubt Mr. Langmire's scientific merits. His various
papers bear ample testimony of a shrewd and well-informed
mind; and the honourable assurances of his practical knowledge in
mines, which I had the happiness to receive from many gentlemen
respectable for their attainments, render the position unques-
tionable. Mr. Children's scientific worth stands pre-eminent in
the lists of human knowledge, and is universally acknowledged.

It is painful to see the asperities of controversy, and particu-
larly those of superior minds. In the warmth of our feelings,
excited by an ardent love of what we esteem and value and ad-
mire, inadvertent expressions escape, which our cool and calmer
moments of reflection condemn, and wish to recall. For myself,
I own I felt hurt when the admirers of this discovery were told
that they were amused with the " novelty of the thing," as if
we were *children,* and the object of our admiration *a Dutch toy;*
and now we are informed that it may be esteemed as a "*curiosity.*"
A little while, and the balance of truth, directed by the hand of
time, will incline the scale, and pronounce a verdict in favour of
the absolute security of Sir H. Davy's safe-lamp, which the
storms of opposition can never disturb, and which will stand un-
shaken in " proud pre-eminence."

In proof of the explanation offered by me, for the principle on
which hinges the offered security,—and that it does not depend
on the *conducting* character of the metallic wire-gauze,—I may
state again, that non-conductors of caloric will cut off the com-
munication of flame. This may be easily proved :—Let a number
of punctures be made in a card, and propel a jet of carburetted
hydrogen through it ; the experiment will decide that the gas
may be ignited on the outer surface of the card, and cut off the
flame from communicating with the orifice of the pipe.

Besides all this, the ambient air is a bad conductor of heat.

I have insulated a flame surrounded by an *atmosphere of
steam,* and found that combustion would not communicate
through it.

Methinks it is cruel to torture the miner's peace by raising
suspicions as to the security of the safety-lamp:—for my own part,
I glory in being the advocate of a discovery so illustrious, and
of one which holds out such important and extensive and lasting
benefits to mankind.

I had

· I had imagined that this lamp would have held precedence of all others, and that there existed no necessity of making a parade about any other, or raising up a rival. I see, however, that Dr. Murray's safe-lamp forms a prominent article in the last number of Dr. Thomson's Annals of Philosophy:—whether the merit of priority rests with Dr. Murray or *with me*, let the following extract from the little volume I published, and quoted by Mr. Hudson, decide : see page 154.

" An air-tight lamp being formed—a pipe might supply it from the mine itself, the orifice of the tube receiving the supply from the stratum of *air contiguous to the floor*,—the carburetted hydrogen being *lighter* and *ascending* would occupy the *upper* part, and thus could not enter the tube."

This is conclusive on the subject,—My Elements of Chemical Science was published June 1815, consequently *five months* before Dr. Murray's paper on the subject appeared and was read before the Royal Society of Edinburgh. Whatever merit then may attach to *priority* in suggesting the structure of an air-tight lamp, to be fed by a flexible tube from the floor of the mine, founded on the specific levity of the ascending carburetted hydrogen, *I must claim as my undisputed right.* But really, sir, I see no use in multiplying safety-lamps, which cannot in their " best estate" compete for a moment with the one introduced by Sir H. Davy: by this " sublimely simple" invention, these imperfect projections are happily superseded ; and for my own part, I should have consigned the suggestion of my mind to forgetfulness, had it not been elicited from me by Dr. M.'s persisting in claiming an exclusive title to this structure ; and but for this, my present statement would have never been. I may again detail to you my plans for getting rid of the fire and choke damps of mines, which has been honoured with *two votes* of thanks from " the Society for preventing Accidents in Coal-mines."—That of the fire-damp will require a plate for its better apprehension.

If my feeble labours in the cause of humanity should be directly or indirectly serviceable, it will delight me to reflect, that at least part of the duties of social and civil life has been fulfilled in me.

<div align="center">I am, with much respect, sir,

Your very obedient and most humble servant,</div>

Surry Institution, Dec. 12, 1816.   J. MURRAY.

<div align="center">*To Mr. Tilloch.*</div>

SIR,—Sir Humphry Davy's claims to a priority of invention in the safety-lamp being established by the unanimous suffrages

<div align="right">of</div>

of a general meeting of the coal trade,—allow me to propose, *seriatim*, the following query:

Has Mr. Langmire made experiments with the wire-gauze safe-lamp,—under what circumstances,—and what were the results ? If in the mine, has it been submitted to that ordeal which it is required to pass, and has it been found wanting ? If in the latter position Mr. L. should answer affirmatively, then he is required to detail the phænomena connected with it, that it may be corroborated or negatived by other experimentalists.

Meantime our faith stands undismayed, for it is seated on a rock.—A mere *ipse dixit*, or bare assertion, unsupported by the proofs of experiment, the philosopher rejects as " trifles light as air."

I am again, with every respect, sir,
Your obliged and very obedient humble servant,
Surry Institution, Dec. 14, 1816. J. MURRAY.

XCIV. *Notices respecting New Books.*

Mr. ANDREW HORN, author of ' The Seat of Vision determined by the Discovery of a new Function in the Organ,' intends publishing a work upon which he has been long engaged,—Illustrations of the Mosaic Cosmogony and Naochian Deluge. It is divided into three parts. The *first* part contains an Inquiry into the origin of the notion prevalent among mankind concerning superior beings; comprising four chapters. I. Concerning an intuitive notion of the Deity. II. On the argument *a priori* for the being of a God. III. On the evidence for a first cause arising from the course of nature. IV. Revelation proved to be the only source from whence mankind have derived their notions concerning the being of a God, and a creation. Part *second*, concerning Philosophical Cosmology, consists of two chapters. I. Opinions of the most celebrated ancient philosophers concerning the origin and constitution of the world.—II. Principles assumed by the most distinguished modern philosophers, to account for the formation and mechanism of the world.—Part *third*, On the Origin of the World, Formation and Revolutions of the Earth according to the principles of Moses; this comprises fifteen chapters.—I. Concerning the antiquity of the world and beginning of things.—II. Of space.—III. Concerning creation and the extent of the Mosaic cosmogony.—IV. On matter, its properties and atomic state.—V. On the reduction of the atomic mass of this earth to a sphere.—VI. An inquiry into the existence and agency of an *universal fluid.*—§ 1. Concerning the *ætherial fluid*, its laws, and evidence that it operates under a *twofold* modification.—§ 2.

On

On the agency of the ætherial fluid in the production of electrical phenomena.—§ 3. Agency of the ætherial fluid in Galvanic phenomena.—§ 4. Agency of the ætherial fluid in magnetic phenomena.—§ 5. Agency of the ætherial fluid in the production of light and colours.—§ 6. On the agency of the ætherial fluid in causing heat.—VII. On the production of the atmosphere.—VIII. Concerning the formation of primitive rocks.—IX. On the production of primitive mountains.—X. On the formation of secondary rocks, and progress of organization from its vegetable state to its perfection in man.—XI. On the physical consequences of man's original transgression.—§ 1. Concerning the nature of the curse imposed upon the ground.—§ 2. On the origin and formation of coal.—§ 3. On the origin of rock salt.—§ 4. Continuation of secondary strata till the ocean ceased to make chemical depositions.—XII. Historic and traditional proofs of the deluge. – § 1. Mosaic account of the deluge.—§ 2. Heathen traditions of the deluge.—§ 3. Proofs from tradition that the poles of the earth were changed at the deluge.—XIII. Natural proofs of the deluge and change in the polar axis of the earth. —§ 1. Longevity of mankind before the deluge.—§ 2. Of the rainbow. —§ 3. Vestigia of the retreat of the waters into the abyss.—XIV. Concerning the partial changes which the earth has sustained, previous and subsequent to the deluge.—§ 1. On the causes that have contributed to the present appearances in the crust of the earth.—§ 2. On the formation of metallic and other veins.—§ 3. On the connection between the changes in the crust of the earth and organic remains found in its strata.—XV. On gravitation, the mechanism of the solar system, and its connection with the universe.—The whole will be comprised in one volume quarto, of about 500 pages, accompanied with four plates illustrative of the various theories and phenomena which it embraces. Among others, the following theories will appear, founded upon a principle *more general* than any hitherto proposed: A new theory of Gravitation, of the Earth, of Light and Colours, of Combustion, &c. &c.

The work will be published by Subscription. The price One Guinea and a Half. It will be put to the Press as soon as the number of Subscribers shall be competent to cover the expense.

Subscriptions will be received by the Author, High Wycombe; Messrs. Gale and Fenner, Paternoster-row, London; A. Constable and Co., and W. Blackwood, Edinburgh.

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* An Inquiry into the Effects of Spirituous Liquors on the Physical and Moral Faculties of Man, and on the Happiness of Society, will shortly be published.

The

The First Part of Volume IV. of the Geological Transactions has just made its appearance. The contents are,

I. Observations on the Geology of Northumberland and Durham. By N. J. Winch, Esq. F.L.S. Honorary Member of the Geological Society.—II. On a Whin Dyke traversing Limestone in the County of Northumberland. By the Hon. Henry Grey Bennet, M.P. F.R.S. Vice President of the Geological Society.— III. Description of an insulated Group of Rocks of Slate and Greenstone in Cumberland and Westmoreland on the east Side of Appleby, between Melmerby and Murton. By the Rev. W. Buckland, Professor of Mineralogy in the University of Oxford, and Member of the Geological Society.

XCV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

The 29th of November, being the anniversary of this Society, the following election of officers took place for the ensuing year.

On examining the lists it appeared that the following gentlemen were elected.

The Right Hon. Sir Joseph Banks, Bart. G.C.B. President.
Samuel Lysons, Esq. Treasurer.
Taylor Combe, Esq. } Secretaries.
William Thomas Brand, Esq. }

Of the Old Council.	*Of the New Council.*
Right Hon. Sir Joseph Banks, Bart. G.C.B.	W. T. Brande, Esq.
John Barrow, Esq.	J. G. Children, Esq.
Samuel Goodenough, Lord Bishop of Carlisle.	J. W. Croker, Esq. M.P.
Taylor Combe, Esq.	Chas. König, Esq.
Sir Humphry Davy, Knt. LL.D.	Alex. MacLeay, Esq.
Sir Everard Home, Bart.	Alex. Marcet, M.D.
Samuel Lysons, Esq.	Colonel Wm. Mudge.
George Earl of Morton.	W. H. Pepys, Esq.
John Pond, Esq. *Astr. Royal.*	George John Earl Spencer.
William Hyde Wollaston, M.D.	Sir John Thomas Stanley, Bt.
Thomas Young, M.D.	

December 5.—Mr. Todd, through the hands of Sir Everard Home, communicated the result of some additional experiments made on Torpedoes at La Rochelle. The subjects he now operated on were one 8 and the other 18 inches long, but they developed no new facts. He found the shocks greater, as might be expected, by improving the conductors, in applying the one hand

to

to the animal, while the other held the scalpel which was brought in contact with the electric organs. The organs, it appears, proceed from the medulla oblongata.

Mr. Hatchett communicated a process for sweetening musty corn, in a letter to Sir Joseph Banks. Several years ago this philosopher was engaged in researches into the quality and products of wheat and barley, in consequence of which he discovered that musty grain, which was so bitter as to be totally unfit for use, and which could scarcely be ground, might be rendered perfectly sweet and sound by simply immersing it in boiling water and letting it remain till the water became cold. The quantity of water in this case was always double that of the corn to be purified. Mr. H. found that the musty quality rarely penetrated through the husk of the wheat, and that in the very worst cases it did not extend through the amylaceous matter which lies immediately under the skin. In the hot water all the decayed or rotten grain swims on the surface, so that the remaining wheat is effectually cleaned from all impurities, and this too without any material loss. The wheat is afterwards to be dried, stirring it occasionally on the kiln, when it will be found improved to an extent which can scarcely be believed without actual experience. The immense quantities of musty corn now in merchants' warehouses or granaries belonging to farmers, render these experiments highly valuable at the present crisis; and it is not doubted that, whatever may be the cavils of ignorance and inhumanity, the good sense of all those interested will lead them to adopt so easy, cheap, and effectual means of rendering their wheat or other grain from 10s. to 40s. a quarter more valuable.

Dec. 12.—The President, happily, was again able to resume his chair, and Mr. Brande read a paper containing the results of his experiments on a species of Chinese galls, which were given to him by Sir Joseph Banks to analyse. It appears that these galls are very valuable for dyeing, or making ink, and that the Chinese use them for their black dyes; they yielded 75 per cent. of the astringent principle, containing oxalic and gallic acid, but no extractive matter. The residuum was chiefly woody fibre. As these galls contain no extractive matter, they are consequently unfit for tanning, and in this respect they differ from all other galls, or even from catechu.

Dec. 19.—The President in the Chair. M. Du Pin, through the hands of Dr. Young, communicated some remarks on naval architecture, and particularly on the improvements adopted by Mr. Seppings. This author began by numerous professions of gratitude for the civilities which he received during his visit to this country, and expressed a wish that the paper then read should be considered as a testimony of that feeling. But science has neither

<div align="right">passions</div>

passions nor country, and the language of adulation or of prejudice is fortunately unknown in the apartments of the Royal Society. M. Du Pin then proceeded by claiming to his countryman, a French shipbuilder, all the merits of Mr. Seppings' improvements, which he alleged were known in France in 1725; he introduced some mathematical calculations, which were of a nature not to be publicly read; and added some remarks on the nature of a ship's arching, or, as our seamen call it, *hogging;* alleging that vessels always arch most just on being launched, and before receiving their guns or cargoes; that their arching augments their capacity at the two ends, that it is not so near the middle as generally supposed, &c., and many other observations, which practical men will treat as mere theoretical dreams, unsupported by the evidence of facts and experience.

With respect to the origin of the improvements made by Mr. Seppings, it is probable that this ingenious mechanician will call for some more substantial evidence, than that of mere assertion. In his first paper which was read to the Royal Society, he mentioned the French vessels which were built on a different plan; he also stated in what his improvements consisted, and how materially they differed from any preceding attempts. He did not indeed pretend that transverse timbers were never before employed. A vessel with vertical planks and horizontal timbers was built at Archangel in the latter end of the 17th century. But it is among the Spaniards and Venetians that we are to look for the earliest and best attempts in naval architecture. The former have always built excellent ships, and near two centuries ago tried the effect of diagonal planks, &c. Hence we may find that all which Mr. S. has adopted from others, was common property in Europe even before the period of 1725.

ROYAL ACADEMY.

Tuesday, Dec. 10, being the forty-ninth anniversary of the Institution of the Royal Academy of Arts, a General Assembly of the Academicians was held at their apartments in Somerset-House, where the following distribution of Premiums took place, viz.

To Mr. Edward Elton, for the best Copy made in the School of Painting, the Silver Medal, and the Lectures of Barry, Opie, and Fuseli, handsomely bound and inscribed.

To Mr. Richard Carruthers, for the next best Copy made in the School of Painting, the Silver Medal.

To Mr. J. C. Leslie, for the best Drawing from the Life, the Silver Medal.

To Mr. Thomas Leverton Donaldson, for the best Architectural Drawing, the Silver Medal,

To

To Mr. Matthew Shepperson, for the best Drawing from the Antique, the Silver Medal.

To Mr. Wm. Behnes, for the best Model from the Antique, the Silver Medal.

The General Assembly afterwards proceeded to elect and nominate the Officers for the year ensuing, when

Benjamin West, Esq. was unanimously re-chosen President.

New Council—A. E. Chalon, W. Mulready, T. Phillips, and M. Shee, Esqrs.

Old Council—Wm. Owen, J. Northcote, Esqrs., Sir William Beechey, and H. Fuseli, Esq.

Visitors in the Painting School—Sir W. Beechey, Sir T. Lawrence; J. Northcote, J. Ward, A. W. Callicot, H. Howard, W. Owen, and T. Phillips, Esqrs.

Visitors in the Life Academy—Sir W. Beechey; W. Mulready, W. Owen, R. Smirke, H. Thomson, J. Flaxman, H. Howard, T. Stothard, R. Westmacott, Esqrs.

Auditors re-elected—G. Dance and J. Farington, Esqrs.

XCVI. *Intelligence and Miscellaneous Articles.*

NEW FRIGORIFIC MIXTURE.

To Mr. Tilloch.

Sir, — Among the numerous mixtures which are attended with a diminution of sensible heat, a very convenient and effective one seems to have hitherto escaped the attention of chemists. It is that of snow and alcohol. The greatest effect appears to take place when equal weights of each are used, and I need scarcely add, that the precaution of effecting the solution in the least possible time is necessary to produce the maximum of cold.

The temperature both of the snow and alcohol being $32°$, the solution in several experiments fell to $-17°$, amounting to $49°$ of Fahrenheit. The alcohol was not of a very low specific gravity, so that I imagine it would not be difficult to produce at least an additional degree. The original communication not having reached you, and no memorandum having been preserved, I cannot now state what its strength was, nor does the season as yet admit of a repetition of the experiment; since the difference in the solubility of pounded ice and of snow will sensibly affect the results.

You will perceive that this circumstance explains the greater degree of cold generated by mixing snow with our strong wines, than by plunging the containing vessel into ice and water.

I am, sir, Your obedient servant,

Blackheath, Dec. 1816.   J. MacCulloch.

PREHNITE DISCOVERED IN GLOUCESTERSHIRE.

Mr. Bakewell has recently discovered Prenhite in a rock of Basaltic Amygdalin at Woodford Bridge, near Berkley in Gloucestershire. This mineral has not, we believe, been found before in any part of England or Wales, though it occurs in Scotland and various parts of the world. The Basaltic Amygdalin of Woodford Bridge contains also decomposing agates, zeolite and green earth. It is further remarkable for having in the upper part large and perfect organic remains of coral, of which Mr. Bakewell brought away a fine specimen imbedded in basalt. The occurrence of marine animal remains in this rock may revive the long agitated question respecting the igneous or aqueous formation of basalt.

STEAM ENGINES IN CORNWALL.

Messrs. Lean, in their report of work done by steam engines in Cornwall, in November, notice an error in their report for October.—For 39,556,496, the work done by Mr. Woolf's engine at Wheal Vor in October, read 40,920,513 pounds lifted one foot high with each bushel of coals.

The counters of some of the engines have been idle in November. The average work of twenty-six reported was 20,538,498 pounds with each bushel. The others reported are:

Woolf's engine at Wheal Abraham, loaded 15·1 per square inch in cylinder, lifted 46,648,718 pounds with each bushel of coals. His other engine at that mine, loaded 3·1 per inch, lifted 21,460,944.

Another engine of his, at Wheal Unity, load 13·8 per inch, appears in this report—Work 34,432,825 pounds lifted with each bushel. An old engine at Wheal Abraham, now under the management of Mr. Woolf, lifted 34,152,162 pounds per bushel.

The altered engine at Wheal Chance, alluded to in our two last numbers, lifted 43,970,893 pounds with each bushel;—load per inch 13·4.

NAUTICAL EPHEMERIS.

Sir,—It seems rather extraordinary that, whilst so many are fruitlessly endeavouring to find *errors* in our excellent Nautical Ephemeris, no one should have noticed the almost entire omission (for the last two years) of the occultations of the planets or fixed stars by the moon. I am the more surprised at this omission, as no cause has been assigned for it; and the notice respecting the notation, as well as the exhortation to mariners and travellers to observe the occultations as often as possible, are still

continued in the general explanation prefixed to the Almanac. At no time were these observations likely to be of more essential service than at the present. From the various anomalies which have occurred in the course of the great Trigonometrical Survey, it appears that the latitudes and longitudes of places, determined astronomically, can no longer be considered as accurate expressions to designate the relative situation of places on the earth. These anomalies are supposed to arise either from irregularities in the figure of the earth itself, or from certain irregularities in the density of its strata, causing a deflection of the plumb-line from the position it would otherwise assume. In either case a careful comparison of the latitudes and longitudes of different places determined trigonometrically, with those of the same places derived from astronomical observations, seems to be the only probable way of throwing light on this intricate subject, and leading us to a true knowledge of the earth's figure.

There is, perhaps, no method of determining the longitude astronomically with greater accuracy, than from the occultations of the fixed stars by the moon. They are also of frequent occurrence; and as there are many individuals in this country who possess the means of making these observations with accuracy, one would wish that every assistance should be afforded that might invite to it. Until our Astronomer Royal shall again afford this, perhaps the insertion in your Magazine of such astronomical phænomena as will occur during the month may not be unacceptable to your readers: if you think so, the inclosed (for January 1817) are at your service, and shall be followed by others in due time.

I am, sir, your obedient, &c.

* * *

Astronomical Phænomena, January 1817.

D. H. M.	
1.12. 5 ☽ 125 ♉	12.15 41 E λ ♎ * 2' N. of ☽ 'scent,
1.16.40 I ⎫ of 132 ♉ * 1' N. of	13. 2. 4 ☽ ♃
1.17.35 E ⎭ ☽ 's centre.	13.20.17 ☽ ♅
2.15.22 I ⎫ of ε ♊ * 8' S. of	14. 4.42 ☽ ♭ Oph.
2.16.26 E ⎭ ☽ 's centre.	14.12.28 ☽ ♂
3.15.41 I ⎫ of κ ♊ * 2' S. of	19. 1. 1 ☽ ♄
3.16.44 E ⎭ ☽ 's centre.	19.19 56 ☉ enters ♒
4.15.48 ☽ γ ♋	20.12.25 ☽ ♀
6. 2.58 ☽ ♌	21.17.18 ☽ 33 ♓
7.23.36 ☽ ν ♍	23 22.48 ☽ ν ♓
9. 2.45 ☽ γ ♍	24. 0. 0 ☽ apogee
9. 0. 0 ☽ perigee	28.21.17 ☽ 125 ♉
10.19.35 ☽ κ ♍	28. 1. 6 ☽ 132 ♉
11.15. 4 ☽ α ♎	30. 0. 6 ☽ ε ♊
12.12. 0 ☽ κ ♎	31. 0.32 ☽ κ ♊

SIR,—The new blow-pipe, acting by a stream of condensed oxygen and hydrogen, has deservedly excited much interest: a detail of some experiments effected by means of this powerful instrument may prove interesting. I am following them up; and may merely mention, meantime, that the oxygen was obtained from *oxymuriate of potassa*, and the hydrogen from *zinc*, &c. The proportions, such as form the constituents of water :

1. PLATINUM as thick as a stocking wire was *instantly fused*, *scintillated*, and fell in a *large globule*.

2. PALLADIUM *fused instantly*, and *slightly scintillated*.

3. A WATCH-SPRING melted with most *splendid coruscations*, fused into a *large globule*, and even BOILED *violently*.

4. Pure caustic ALUMINA and MAGNESIA burnt with *indescribable brilliancy*, exhibiting a *splendour of light rivalled only by the sun*.

5. Part of a TOBACCO-PIPE *burnt vividly*, and was fused into GLASS.

6. A piece of INDIGO exhibited a *beautiful and intense flame*.

7. A fine *electric* TOURMALIN grew *red hot, instantly fused*, and *flamed*.—It did not *forgo* its *electric powers*.

8. The DIAMOND, in a groove of charcoal, was submitted to its influence :—In a short time it became *red hot*, then BURST INTO FLAME; and, when dislodged from its nidus, it fell upon the table, and continued a *second or two in actual flame*.

Through Dr. Clarke, the chemist has received an agent of the most extraordinary powers of ignition. Anticipation augurs in " breathless expectation" the most brilliant results. Man is only in the infancy of his being !

<div style="text-align:center">I am, respectfully, Sir,
Your most humble servant,</div>

Surry Institution, Dec. 21, 1816.   J. MURRAY.

P.S. A mass of PERCARBURET of iron (plumbago) gave beautiful *minute sparks*, and was *fused*.

ROCK CRYSTAL *decrepitated violently*. At this moment an explosion of the condensed gases took place, and the instrument was rendered useless.

This accident suspends for the present a continuation of the experiments ; but the chemist must smile at danger when such brilliant results rise up in prospective.   J. M.

REFRACTION.

SIR,—It has often occurred to me, on reading Sir Isaac Newton's 14th query at the end of his " Optics," that some interesting

ing experiments might be made on the refraction of light through media of different densities.

Besides glass and water and atmospheric air, the rays of light might be directed through vessels of varied sizes and shapes, filled with alcohol, ether, naphtha, or even the various gaseous substances, from the heavy carbonic acid gas to the lightest hydrogen.

In these experiments the rays of light might also be examined when separated into their different colours, and thus directed through these various media.

Perhaps these hints may induce some of your correspondents, who have the leisure and the conveniencies requisite for the performance of such experiments, to engage their attention to them, and communicate the result to the public. As to myself, I can only say—*Fungar vice Cotis.*   *A Correspondent.*

*** Experiments of the kind suggested by our correspondent have been made, and have even been carried so far that the results were attempted to be applied some years ago to the improvement of achromatic telescopes by a gentleman in Edinburgh, though we have not heard with what success. As, however, his suggestion may induce other experimentalists to turn their attention to the subject, we have given his well-meant communication a place in our pages.

ELECTRICAL AND PHILOSOPHICAL APPARATUS.

The excellent collection of electrical apparatus and other philosophical instruments, which have been employed by Mr. Singer in his Lectures, is to be offered to public sale early in the present season, in consequence of the discontinuance of those lectures from the illness of Mr. Singer.

LARGE EUHARMONIC ORGAN.

Messrs. Flight and Robson of St. Martin's Lane have completed a large and fine Organ, for the East Indies, with compound stops, the first of such which have yet been made, on the Rev. Henry Liston's patent plan; in which instrument separate pipes are provided for every sound (near 60 in each octave) in all the upper parts of the scale, and *shaders* for producing two or three sounds (differing by comma major) from the same pipe, are only used in some of the larger ranges of pipes, both for saving of room, and because it has been found by experience, that in such lower parts of the scale, the shaders act the best. In a short time Mr. Liston proposes to commence a Course of Lectures on the musical scale, as now in use by singers, violinists, &c., illustrated by experiments on all the chords in use, and by performances, on this large organ.

EXPEDITIONS TO AFRICA.

Melancholy accounts have, we are sorry to say, been received, respecting these expeditions. There are two :—the one intended to penetrate from Senegal through the Deserts to the banks of the Niger ; the other to proceed by steam-boats up the Congo, under the idea that the former empties its waters into the Atlantic through the latter great river, on the banks of which the two expeditions expected to meet ;—the former under the directions of Major Peddie ; the latter commanded by Captain Tuckey, who proceeded sooner than had been generally reported. Major Peddie was at Senegal the end of September. The following is an extract of a letter from the agent to Lloyd's, dated Bahia, October 30 :—" You will observe by the list herewith, the arrival of the Congo and Dorothy, two ships fitted out on the coast of Africa for a voyage of discovery ; they have put in here from the opposite coast, in consequence of the sickly state of their remaining crews, nearly two-thirds having died of the fever, including the commander, first-lieutenant, and botanists."

IMPROVED METHOD OF MAKING BREAD.

Mr. Edmund Davy, of the Cork Institution, has communicated the following important facts to the public in *The Munster Farmer's Magazine.*

" The carbonate of magnesia of the shops, when well mixed with the new flour, in the proportion of from twenty to forty grains to a pound of flour, materially improves it for the purpose of making bread. Loaves made with the addition of the carbonate of magnesia rise well in the oven ; and after being baked, the bread is light and spongy, has a good taste, and keeps well. In cases when the new flour is of indifferent quality, from twenty to thirty grains of the carbonate of magnesia to a pound of the flour will considerably improve the bread. When the flour is of the worst quality, forty grains to a pound of flour seem necessary to produce the same effect.

" As the improvement in the bread from the new flour depends upon the carbonate of magnesia, it is necessary that care should be taken to mix it intimately with the flour, previous to the making of the dough.

" I have made a great number of comparative experiments with other substances, mixed in different proportions with the new flour. The fixed alkalies, both in their pure and carbonated state, when used in small quantity, to a certain extent improve the bread from the new flour ; but I have found no substance so efficacious in this respect as the carbonate of magnesia.

" The greater number of my experiments have been performed

on the worst new seconds flour I could procure. I have also made some trials on seconds and firsts of different quality. In some cases the results have been more striking and satisfactory than in others—but in every instance the improvement of the bread, by the carbonate of magnesia, has been obvious. It may be necessary to remark, that, in all my experiments, a proper attention was paid to all the circumstances connected with the accuracy of the results. The materials were weighed and mixed together, the dough was made up, and the bread baked, under my own inspection.

"The trials I have made have been necessarily on a small scale; they have been performed chiefly on quantities of flour not exceeding a pound in weight. I shall state the results of a comparative trial on the worst new seconds I could get, with and without the addition of the carbonate of magnesia.

"I made five small loaves, each containing one pound of flour, one hundred grains of common salt, and a large table-spoonfull of yeast. The dough, in all of them, was made up with water at the temperature of 100 deg. Faht. and exposed before the fire for two hours at the temperature of 70 deg. to ferment.

The first loaf contained no other addition.

The second .... 10 grains of carbonate of magnesia.
The third .... 20 grains .... do .... do.
The fourth .... 30 grains .... do .... do.
The fifth .... 40 grains .... do .... do.

"After they had been all baked together in the same oven, the loaves when cold were examined.

"The loaf without the carbonate of magnesia had fallen in the oven; it was like a cake, and was soft and clammy, and readily adhered to the knife.

"The loaf with the addition of ten grains of the carbonate of magnesia was improved; it had risen better than that which contained none, but still the improvement was not very considerable.

"The loaf with twenty grains was far superior to the one with ten grains; it was for the most part light and porous; but still there was a slight tendency to heaviness. The loaf with thirty grains was still better than the one with twenty grains. But the loaf with 40 grains was uniformly light and spongy, of a more regular texture and better colour than any of the others.

"In all my experiments I have used the porter yeast, but a successful trial has been made with the new barm. I have never made any secret of the foregoing facts, but have readily communicated them to a number of gentlemen, to whom I have shown specimens of the bread made with, and without, the carbonate of magnesia, and they have uniformly given their assent to this improvement.

"I un-

" I understand a number in Cork and in the neighbourhood have been induced to repeat my experiments; and they have, for the most part, been attended with great success. If there should be any instances of failure, it would not be proper hastily to refer them to a defect in the method, but to impute them rather to a want of attention to those circumstances necessary to insure success.

" I conceive not the slightest danger can be apprehended from the use of such an innocent substance, as the carbonate of magnesia, in such small proportion as is necessary to improve bread from the new flour. It is well known to be administered with perfect safety, even to infants. In order to try the effect of bread made with this substance on myself, I have used it exclusively for the last five weeks, without the least inconvenience, in the proportion of sixty, eighty, and even one hundred grains, to a pound of flour.

" A pound of carbonate of magnesia would be sufficient to mix with two hundred and fifty-six pounds of the new flour, at the rate of thirty grains to the pound. And supposing a pound of carbonate of magnesia to cost half-a-crown, the additional expense would be only half a farthing in the pound of flour.

" I am not quite satisfied as to the peculiar agency of the carbonate of magnesia, in correcting the bad quality of new flour. It is an extremely light substance, and may probably tend to improve the texture of the bread. The new flour, too, in the process of baking, may perhaps be disposed to undergo the acetous fermentation, and the carbonate of magnesia, being slightly alkaline, may correct the tendency of the fermented dough to acidity. On this part of the subject, however, the experiments I am now pursuing will, I trust, throw some light.

I am, dear sir, yours truly,

" Cork Institution, Dec. 3, 1816.   EDMUND DAVY."

Continuation of M. Van Mons's Intelligence.

I have learnt from Messrs. Gay Lussac and Arrago, that the system of chemistry and the names of substances are experiencing a new revolution, and that in a short time there will not remain one stone upon another of the existing edifice.

M. Dulong has made some experiments upon nitric acid, which fully confirm what I long since made known, that the fuming nitric acid is simply nitric acid with nitric vapour interposed, in the same manner as the fuming sulphuric acid consists of sulphuric acid with sulphuric vapour interposed. This vapour when set apart becomes crystallized : Dulong has found that the nitric

pour is liquefied by cold, and becomes coloured; he might without doubt by a cold more intense have crystallized it. You will recollect that in the report of my experiments with a cold of 58° R. I observed that the nitric and muriatic acids, in congealing, produced two sorts of crystalline concretions, which when liquefied take a red colour in the nitric acid and a greenish in the muriatic. I mentioned further, that sulphuric acid dashed with nitric vapour, loses its colour before congealing, and recovers it on being liquefied: that colour is considerably deepened when the fuming sulphuric acid is heated in close vessels. I propose during this winter to submit to the same degree of 58R. the different compound gases; for which purpose I have still all my apparatus and a great store of muriate of lime.

———————

M.. Virey in a communication to the French Academy of Sciences (formerly the Institute) states that the spur of the rye is not a *champignon* of the genus *Scleroticum*, as M. Decandolle had endeavoured to prove; but that it is a real disease of the grain; since there are to be found in it all the peculiarities of organization of the rye, a degeneration as yet unknown in its nature, amylaceous fecula, and probably all the immediate materials of the Cerealia.

———————

Messrs. Beauvois, Thouin, and Thenard have also made a report to the Academy on two papers of M. Dupetit Thouars, relative to the effect produced by frost on flowers and young shoots. M. Dupetit Thouars seems, according to the report, to have been the first who perceived a small icicle even in the substance of the calyx of some flowers; but almost all botanical authors had already noticed this phænomenon in the young buds. " In spring," observes M. Sennebier, " the new shoots of the ligneous plants are extremely tender, humid, and full of aqueous juices: the frost then destroys them with the herbaceous plants, because the water which is frozen occupies a greater space than in the fluid form. Its sudden expansion destroys the frail organization of the vessels which contain this water: but these alterations are more or less disastrous, according to the nature of the organs, and their parts. Thus, if those organs and those fibres were susceptible of a great expansibility, if they were at the same time very elastic, and that they could resume their first state as soon as the water thawed, then the dilatation of the water changed into ice will dilate the organs of the plants, and they will resume their first form, without having preserved any apparent trace of alteration." The committee are of opinion, in this way we may account for the little action which

frosts

frosts seem to have sometimes, according to M. Dupetit Thouars, upon the young shoots, and on the calices of the flowers.

LIZARDS FOUND IN A CHALK ROCK.

Dr. Wilkinson lately presented to the Bath Philosophical Society a letter he had received from a clergyman in Suffolk, relative to two lizards being discovered by the rev. gentleman in a chalk rock, with some interesting circumstances tending to explain why all the animals which have been discovered in rocks, marbles, &c. die on their exposure to the atmosphere. From observations made, there appeared to be some obstruction in their respiratory organs. One being placed in water disengaged itself from this obstruction; while the other died, from not being enabled to liberate itself from the viscous matter lining the throat. The clergyman in his letter says : " A pit having been opened in the summer of 1814, at Elden, Suffolk, for the purpose of raising chalk, I deemed it a favourable opportunity for procuring fossils; accordingly commissioned the men employed to search for and reserve whatever appeared curious. In this search I sometimes assisted, and had the good fortune to be present at the discovery of two lizards imbedded in the solid chalk, fifty feet below the surface. The following is the result of my observations:—So completely devoid of life did the lizards appear on their first exposure to the air, that I actually considered them in a fossil state : judge then of my surprise when, on my attempting to take them up, I perceived them move! I immediately placed them in the sun, the heat of which soon restored them to animation. In this state I carried them home, and immersed one in water, keeping the other in a dry place. You may perhaps consider it worthy your observation, that the mouths of the lizards were closed up with a glutinous substance. This obstruction seemed to cause them great inconvenience, which was evident from the agitation perceptible in their throats, and from the frequent distension of the jaws, or rather around the jaws and the head ; indeed they seemed in a state little short of suffocation. The newt which had been immersed in water, after many violent struggles, was at length enabled to open its mouth: this afforded it instant relief, and it evidently derived much satisfaction and comfort from its new element. The other lizard, notwithstanding its repeated endeavours, was unable to open its mouth. It died in the course of the night, probably from being debarred the use of its proper element. The remaining lizard continued alive in the water for several weeks, during which it appeared to increase in size. It disliked confinement; and after many attempts, at length, to my great mortification, effected its escape, nor could I ever after find it,

CHEMICAL TESTS AND APPARATUS.

We are happy to inform experimentalists and scientific persons resident in the North of England, that they may be supplied with Chemical Reagents in a state of purity, subjects of experiment, and apparatus for chemical and philosophical pursuits, by Mr. William West, chemist, Leeds.

Mr. Taunton's Winter Course of Lectures on Anatomy, Physiology, Pathology, and Surgery, will commence on Saturday, January 18, 1817, at Eight o'clock in the Evening *precisely*, and be continued every Tuesday, Thursday, and Saturday, at the same hour, at the Theatre of Anatomy, 87, Hatton Garden.

Meteorological Observations kept at Walthamstow, Essex, from November 15 *to December* 15, 1816.

[Usually between the Hours of Seven and Nine A.M.]

Date. Therm. Barom. Wind.

November

15	29	29·50	W.—Clear and clouds; *cumulostratus*; fine day; star-light.
16	29	29·72	NW.—Gray morning; windy; fine cold gray day; very dark night.
17	31	30·01	NW.—Clear and *cirrus*; fine sunny cold day; dark night.
18	47	29·71	S.—Hazy; showers; rainy; star-light.
19	31	29·72	W.—*Cirrus* SE; *cirrostratus* NW; eclipse of the sun visible; fine day; dark night. New moon.
20	44	29·94	S.—Hazy; *cirrus* NW.; sun and clouds'; dark night.
21	41	30·00	S.— *Cirrus* and *cirrostratus*; wind and cloudy; some sun; dark night.
22	38	29·92	E by S.—Gray; fine sun and windy day; star-light.
23	29	29·72	E.—White frost; gray fine cold day; star-light.
24	19	29·83	E.—Sun; white frost, and hazy; very fine sun; fog; star-light.
25	29	29·83	E.—White frost and clear; fine day; hazy at 2 P.M.; rainy evening.
26	42	29·83	S.—Fog and rain; very rainy and foggy at noon; *cirrostratus*, and less rain; cloudy night. Moon first quarter.
27	35	30·21	NW.—Sun through fog; clear and clouds; moon through *cumuli*.
28	43	30·32	W.—Gray; gray day; fine sun, but foggy.

November

Date. Therm. Barom. Wind.

November

29   34   30·43   W.NW.—Foggy; sun through fog ; fine day; moon and star-light ; but a little hazy.

30   30   30·61   NW.—White frost; very fine day; barometer 30·68 at Clapton, one part of the day; light, but neither moon nor stars visible.

December

1   34   30·61   N.—Gray; fine gray day; clear moon and star-light.

2   34   30·44   N.—Clear and clouds; gray day; light, but no moon nor stars.

3   31   30·43   N.—Foggy; *stratus;* but clear above; fine clear day; (great fog in London;) light, but hazy.

4   37   30·40   N.—Cloudy ; hazy at eight ; gray day; visible eclipse of the moon, but not seen till above half over, because of the clouds about 10 P.M.—Full moon.

5   40   30·01   SE.S.—Gray; fine gray day; showers and wind, after dark.

6   39   29·51   SW.W.— Fine moon-light morning; fine sunny day; *cumuli* and moon.

7   29   29·41   NW.W.—Clear and windy ; fine day; hazy at 3 P.M.; clear moon and star-light.

8   33   29·51   S.—White frost, and foggy ; sun through fog; some showers, sun and wind ; clear moon and star-light.

9   29   29·65   W.—White frost; clear and *cirrostratus;* clouds and sun ; cloudy and windy,

10   39   29·50   SW.— Clear and clouds; *cirrus* S; fine day; dark and windy.

11   39   29·43   SW.—Foggy; sun, and *cirrostratus;* star-light; snow in the night.

12   32   29·43   SE.—Some snow on the ground; foggy; very rainy after 11 A.M. till dark; star-light.—Moon, last quarter.

13   38   28·93   W.— Clear and clouds; sun, and great showers and wind ; star-light.

14   34   29·43   W.—Foggy; fine day; dark and rainy.

15   43   28·82   SW.—Windy; clear and clouds; fine sun and wind; *cumulostratus;* fine day; star-light.

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LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1816.	Age of the Moon	Thermo-meter.	Baro-meter.	State of the Weather and Modification of the Clouds.
	DAYS.			
Nov. 14	25	40'	29·85	Fair—storm of rain and snow in the evening—frost
15	26	36·	29·60	Very fine—snow in the evening
16	27	36·5	29·94	Stormy—snow, sleet, and rain—wind N.
17	28	37·5	30·20	Fair—snow in the evening—heavy rain all night
18	29	46·	29·75	Rainy—fair—rime frost at night
19	new	41·	29·90	Fair
20	1	49·	30·10	Very fine
21	2	49·5	30·15	Ditto
22	3	39·	30·01	Ditto—frost at night
23	4	35·	29·95	Fair
24	5	35·	30·	Very fine
25	6	36·	30·	Ditto
26	7	42·	30·10	Cloudy—thick fog at night, and rain early next morning
27	8	46·	30·29	Ditto
28	9	41·5	30·40	Ditto—fog—frost at night
29	10	43·	30·59	Very fine—ditto
30	11	36·	30·76	Cloudy—ditto
Dec. 1	12	38·5	30·74	Very fine—ditto
2	13	42·5	30·52	Fair—rain at night
3	14	39·	30·50	Ditto
4	full	40·5	30·48	Ditto
5	16	41·	30·08	Ditto—heavy rain at night
6	17	42·	29·62	Very fine—frost, snow, and rain at night
7	18	37·	29·58	Ditto ditto
8	19	39·	29·70	Rain ditto
9	20	38·5	29·69	Fair—heavy rain at night
10	21	39·	29·65	Very fine ditto
11	22	39·5	29·32	Ditto [frost at night
12	23	37·	29·37	Cloudy—heavy rain in even[g]—rime
13	24	37·	29·22	Rain, snow—wind—frost at night
14	25	40·	29·51	Very fine

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For December 1816.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.			
Nov. 27	32	47	47	30·21	22	Fair
28	45	45	40	·24	10	Cloudy
29	35	45	39	·32	15	Fair
30	36	39	35	·49	26	Fair
Dec. 1	37	39	33	·49	16	Cloudy
2	33	40	40	·33	10	Cloudy
3	37	41	38	·30	0	Foggy
4	38	40	40	·20	0	Cloudy
5	40	42	39	·08	0	Cloudy
6	40	43	39	29·50	10	Fair
7	36	40	40	·45	12	Fair
8	35	40	36	·49	0	Showery
9	33	44	40	·58	12	Fair
10	40	48	46	·20	0	Stormy
11	40	43	36	·20	5	Fair
12	32	37	48	28·90	0	Stormy
13	37	42	35	29·15	0	Stormy
14	34	42	42	·05	15	Fair
15	40	44	36	28·82	0	Stormy
16	33	40	38	29·45	10	Fair
17	40	49	40	·22	0	Rain
18	39	42	38	·56	6	Cloudy
19	34	36	29	30·36	6	Fair
20	27	35	27	·41	10	Fair
21	25	32	24	·15	12	Fair
22	23	28	25	·02	10	Fair
23	32	42	46	29·75	7	Cloudy
24	47	50	44	·51	6	Cloudy
25	39	43	46	·70	13	Fair
26	48	49	40	·30	0	Stormy

N.B. The Barometer's height is taken at one o'clock.

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END OF THE FORTY-EIGHTH VOLUME.